EXPERIMENTAL NEUROSES AS LEARNED BEHAVIOUR

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I. INTRODUCTION

In a recent issue of this *Journal* Russell (39) presented a very lucid review of experiments in which neuroses have been artificially produced, and of the theories that have been put forward in explanation. He did not pronounce on the relative merits of the theories, as he felt that further experimental and logical studies were prerequisite. In the present paper a new analysis is made of previously reported experimental neuroses; and then some new experiments are described which, it is believed, clarify our understanding of the mechanisms by which these conditions are produced. The study as a whole supports the conclusion that experimental neuroses constitute a variety of learned behaviour.

Definition

An animal is said to have an experimental neurosis if it displays unadaptive responses that are characterized by anxiety, that are persistent, and that have been produced experimentally by behavioural means (as opposed to direct assault on the nervous system by chemical or physical agencies such as poisonings or extirpations).

An *adaptive response* is defined by Warren (41) as 'any response which is appropriate to the situation, i.e. which favours the organism's life processes'. This implies that it is a response that has the *effect* of leading directly or indirectly to the reduction of the organism's needs or to the prevention of pain or fatigue. An *unadaptive response*, on the other hand, does not lead to either.

II. ANALYSIS OF PREVIOUS EXPERIMENTAL FINDINGS

Russell (39), like previous reviewers (5, 22), seems to have been too ready to accept without question the interpretations the various experimenters have given of their own work. It will be seen in the paragraphs that follow that these interpretations often ignore important

factors in the experiments, and that if these factors are consistently taken into account all the neuroses so far recorded can be understood as being the outcome of one or other (perhaps sometimes both) of two basic situations—the exposure of the organism to ambivalent stimuli or its exposure to noxious stimuli, in either case under conditions of confinement. It will be argued that these two kinds of situation have an effect in common upon which the development of the neuroses depends.

In the account that follows the neuroses are grouped according to the basic situation which seems to have determined their production, and it will be seen repeatedly how experimental procedures may differ widely in other respects and yet provide the same kind of basic situation.

(i) *Neuroses produced by situations of ambivalent stimulation*

An *ambivalent stimulus situation* is a situation to which opposing responses tend to be elicited simultaneously in more or less equal measure. The range of meaning is narrower than would be implied by 'conflict situation', for not in every conflict are the opposing response tendencies equal. Examination of the literature reveals that three varieties of situations involving ambivalent stimulation have been effective in producing experimental neuroses. These will be considered under their headings below.

(*a*) *Neuroses produced by difficult discriminations*

Although the expression 'difficult discrimination' carries an unwanted suggestion of conscious weighing-up, it is a conveniently brief term of reference for the ambivalent stimulus situation that occurs under the following circumstances: a positive and a negative response having respectively been conditioned to two stimuli at different points on a continuum, the animal is confronted with a stimulus at an intermediate point on the continuum such that the opposing responses are evoked in more or less equal strength.

Pavlov's experiment on neurosis-production based on difficult discrimination was the first to be reported ((36), pp. 290–1). The projection of a luminous circle on to a screen in front of a dog was repeatedly followed by feeding. When the alimentary response to the circle was well established, an ellipse with semi-axes in the ratio of 2:1 began to be projected among presentations of the circle, and was never accompanied by feeding.

> A complete and constant differentiation was obtained comparatively quickly. The shape of the ellipse was now approximated by stages to that of the circle (ratios of semi-axes of 3:2, 4:3 and so on) and the development of differentiation continued...with some fluctuation, progressing at first more and more quickly, and then again slower, until an ellipse with ratio of semi-axes 9:8 was reached. In this case although a considerable degree of discrimination did develop, it was far from being complete. After three weeks of work upon this differentiation not only did the discrimination fail to improve, but it became considerably worse, and finally disappeared altogether. At the same time the whole behaviour of the animal underwent an abrupt change. The hitherto quiet dog began to squeal in its stand, kept wriggling about, tore off with its teeth the apparatus for mechanical stimulation of the skin, and bit through the tubes connecting the animal's room with the observer, a behaviour which never happened before. On being taken into the experimental room the dog now barked violently.

Because of its close resemblance to the circle the 9:8 ellipse presumably generated at least as great a positive alimentary response tendency due to primary stimulus generalization ((18), p. 184) from the circle as negative alimentary response tendency due to generalization from previous ellipses. Since primary stimulus generalization depends on the common neurones activated by similar stimuli (47), if the 9:8 ellipse activated a preponderating

number of neurones in common with the circle, the negative response tendency to the ellipse could never become completely dominant, however often it were presented to the animal without reinforcement. Consequently, this ellipse would always evoke ambivalent response tendencies.

Other workers who have produced neuroses by difficult discriminations that are visual are Bajandurow (4), who subjected doves to a circle-and-ellipse experiment similar to Pavlov's (although, it may be noted, he employed a conditioned avoidance response—which, as will be seen later, confuses the causal relations); and Jacobsen, Wolfe & Jackson (20), whose chimpanzee developed neurotic reactions in the following manner. The animal was allowed to observe the experimenter conceal a piece of food under one of two cups. An opaque screen was then lowered so as to hide the cups from her view. After an interval it was raised so that she could approach the cups and obtain the food if she lifted the correct one. In a first series of trials, the cups were separated by 30 in., and the chimpanzee was able to go to the correct one even after the screen had been lowered for from 3 to 5 min. In a second series, the distance between the cups was 10 in., and she then failed to recognize the cup covering the food. Normally highly emotional, she became profoundly upset whenever she made an error. Subsequently, she would watch the loading of the cup closely, and would often whimper as the cup was placed over the food. As the opaque screen was being lowered, she would fly into a temper tantrum, roll on the floor, micturate and defecate. After a few of these reactions she ceased to make responses to this test, though continuing to respond eagerly to other stimuli. Testing was continued daily for 3 weeks. By the end of this time she had to be taken by force to the experimental cage. It seems reasonable to suppose that there was so much inter-generalization of the responses to the approximated cups that the animal responded both positively and negatively more or less equally to each cup; and this is what would constitute an ambivalent stimulus situation.

Several investigators have produced neuroses by means of difficult discriminations of an auditory kind. Gantt (16), using an experimental set-up much like that of Pavlov, made an ascending pair of tones a positive conditioned stimulus to eating, and the same pair in descending sequence a negative stimulus. Behaviour disturbances were produced in dogs by narrowing the frequency difference between the two tones, so that, for example, one was eventually 21 cycles and the other 20. (It is worth remarking, however, that the dog Nick was probably hypersensitive to certain kinds of stimuli from the start, because the first time an artificial stimulus was presented to him (a tone of 1130 cycles for 10 sec.) he was disturbed enough to refuse food until the experimenter entered the camera; and the next day was 'terribly excited by the stimulus'.) Dworkin, Raginsky & Bourne (11) used similar methods to bring about long-lasting neuroses in two dogs, except that discrimination depended on the difference in frequency between single tones, one positive and the other negative. Krasnogorski (23) applied similar techniques to several children, and obtained effects comparable to those observed in the animals. The findings in the above-mentioned experiments are so clearly parallel to those of Pavlov's circle and ellipse experiment that no separate comment is necessary.

Karn (21) has described the production of a neurosis in a cat by a method that is of special interest in that a difficult discrimination of *intraorganismal cues* was involved. The animal was trained in a maze that was in effect a T-maze, with the left turn followed by two further left turns, and the right turn by two further right turns, all

segments being the same length as the stem of the T. The only place where food was ever given was at the end of the final segment. The animal was required to learn at the choice point the double alternation—right, right, left, left. After 230 trials of the sequence it attained an average of 90 % correct in the last 30 trials. During the 232nd trial, at its second arrival at the choice point, the animal hesitated much longer than usual, and then raced to the right end point. During the remainder of the trial it moved slowly, whimpering. From this time onwards the cat resisted being put into the maze, and mewed loudly and micturated at the choice point, especially at the second of the two occasions for a right turn. At later trials the signs of disturbance seemed to get worse, and accuracy of performance became progressively poorer. (In a later article, Karn (22) states that he and E. R. Malamud have produced a neurosis in a dog by the above technique, and that Keller has done likewise with a rat.)

It is reasonable to interpret the results as follows. Each time the cat arrived at the choice point the external cues were the same, and these were equally conditioned to a turn in either direction. In the case of a first arrival at the choice point the distinctive cue of a completely empty stomach must have become strongly conditioned to right-turning. Difficulty would occur at the second arrival, for the internal cues conditioned to right-turning would be the proprioceptive cues from the right-turning movements of one run, plus cues consequent on having eaten once; while conditioned to left-turning would be the proprioceptive cues from the right-turning movements of two runs plus the internal cues consequent on having eaten twice. It is easy to believe that the difference between these two combinations of internal stimuli is minimal, so that at each second (and third) arrival at the choice point the right- and left-turning tendencies would be almost equal, constituting an ambivalent stimulus situation.

A special case of the production of neuroses by difficult discrimination seems to be provided by the use of *threshold stimuli* in experiments described by Dworkin (9) and Dworkin, Baxt & Dworkin (10). Both dogs and cats were used. In conditions of constricted space, the animals were trained to raise the lid of a food-box in response to pure tones of varying pitch. Over several months the loudness of the tones was gradually reduced. At first the behaviour of the animals was normal. But in later experiments one or two loud tones would be used at the beginning of each session, and the loudness would then be progressively reduced to below threshold. The majority of animals of both species underwent changes in behaviour, which in a few cases were very marked. The changes comprised increasing failure of response to conditioned stimuli, refusal to eat at all within the conditioning room, and, in the case of the cats, excessive general bodily movement. Three cats manifested severe symptoms that lasted for months (10). In these experiments the difficult discrimination seems to be discernible as follows. The experimental environment *with* the auditory stimulus stimulates approach responses to the food-box. *Without* the auditory stimulus the environment has an inhibitory effect. A threshold stimulus provides intermediate conditions by which both responses tend to be evoked at once.

(*b*) *Neuroses produced by increasing the delay before reinforcement of a delayed conditioned response*

In Pavlov's laboratory ((36), pp. 293–4), a dog with a predominant tendency of excitation had become conditioned to make the alimentary response to any of six stimuli. Food was presented when a stimulus had been acting for 5 sec. The duration of action of

the conditioned stimulus before presentation of food was now prolonged by 5 sec. daily. All six stimuli were treated in this manner concurrently. When the delay reached 2 min., 'the animal began to enter into a state of general excitation, and with a further prolongation of the delay to 3 min. the animal became quite crazy, increasingly and violently moving all parts of its body, howling, barking and squealing intolerably. All this was accompanied by an unceasing flow of saliva....'

This neurosis is regarded as produced by ambivalent stimulation because of the following considerations. Pavlov ((36), p. 92) found that when a delayed conditioned reflex has been established, salivation is delayed until some time after the commencement of the conditioned stimulus. Presumably, then, certain durations of the conditioned stimulus have become conditioned to an inhibition of the alimentary response, and other, longer durations to an excitation of that response. If the delay until presentation of food is now increased in stages, at each stage a duration of the stimulus which had previously stimulated the response would begin to be conditioned to an inhibition thereof. Whenever the positive and negative tendencies were more or less equal the stimulus would be an ambivalent one according to the definition given above.

(c) *Neuroses produced by rapid alternation of stimuli eliciting opposing responses*

In the two types of ambivalent stimulus situation just described the ambivalent effects are produced in the last resort by a single stimulus. The situation would be quite similar if the two opposing responses were elicited each by its own stimulus, provided that the stimuli followed each other rather closely. In one of Pavlov's dogs, among other positive and negative alimentary conditioned reflexes, conditioning was established positively to a tactile stimulation of 24 per minute and negatively to 12 per minute. One day the positive rate was made to follow the negative without any interval. For the first few days after this it was found that all positive conditioned reflexes had disappeared. This was followed by a period in which there were changing relations between the magnitudes of response to the various stimuli. The disturbances are said to have lasted $5\frac{1}{2}$ weeks, but neither in Anrep's translation ((36), pp. 301–2) nor in Gantt's ((37), pp. 343–4) are any other changes in behaviour described.

(ii) *Neuroses produced by noxious stimuli*

A noxious stimulus is one that causes tissue disturbance of a kind that leads or tends to lead to withdrawal behaviour. In man it is correlated with the experience of pain or discomfort.

In certain experiments in which noxious stimuli have been used to produce neuroses a 'clash' between excitation and inhibition has been involved in one way or another. In other experiments, feeding reactions have been interfered with by the presentation of the noxious stimulus. Because in both categories conflict is in some sense present it has been regarded as the centre point of the aetiology in both. Nevertheless, these neuroses are grouped here among those due to noxious stimuli and not to ambivalent stimulation; the first category for reasons given below in the course of an examination of the experiments, the second for the following reasons. First, as was shown above, to result in a neurosis, conflicting tendencies must be of approximately the same strength at the same time, whereas when the reaction to a noxious stimulus has interfered with feeding it has been overwhelmingly stronger than the feeding tendency. Secondly, in experiments

to be described below, the writer has found that noxious stimuli, alone and without the aid of any conflict, can be an entirely adequate cause of neurotic reactions. If anything, a more severe neurosis tends to be produced by shock alone than by shock associated with feeding.

In the following account of the various procedures that have been employed more specific comments will be made. To unify the commentary the procedures are grouped according to what the experimenter has believed to be the crux of his method.

(*a*) *Noxious stimuli in experiments involving 'clash' between excitation and inhibition*

The most extensive series of experiments coming under this heading has been reported from the Cornell Behaviour Farm by Liddell & Bayne (25), Anderson & Liddell (1), Anderson & Parmenter (2), and Liddell (24). Sheep, goats, dogs and pigs were used. It will be noticed that some of the experiments parallel those of Pavlov, mentioned above, among the ambivalent stimulation neuroses, but the 'unconditioned response' is a shock-avoidance reaction in place of the alimentary reaction. The basic procedure is described as follows by Anderson & Liddell (1):

> The sheep to be conditioned was led to the laboratory. It ascended a platform and stood on a table where it might eat from a basket of oats. Its freedom of movement was restricted by loops passing under the legs and attached to a beam overhead. With the incentive of food at the beginning and end of the experiment, the sheep within a few days would run on leash to the laboratory from the barn, mount the table and remain quietly for as long as two hours. A leather bracelet wrapped with brass wires was attached to the shaved skin of the upper part of the foreleg....A brief tetanizing shock, not painful to the experimenter's touch...evoked a brisk flexion of the sheep's foreleg, after which the animal became quiet....If the shock was regularly preceded by some neutral stimulus such as the ticking of a metronome, soon the ticking of the metronome would elicit movement of the leg in anticipation of the shock....The signal that a shock was coming was invariably followed by the shock. The animal quieted down within a few seconds after the shock had been administered.

The variations of method used to produce neuroses in the animals are illustrated below by summarized case histories. The headings indicate the *experimenters'* view of what is relevant.

Subjecting the animal to a difficult discrimination. 'Sheep 3' in Anderson & Liddell's article (1) learned to raise its foreleg to a metronome of 120 beats per min. when that rhythm was constantly followed by a shock. A metronome of 50 beats per minute was never accompanied by shock, and soon ceased to evoke the flexion response. Rates of 60, 72, 84 and 92 beats were then in turn successfully made negative signals, but 100 beats could not be differentiated from 120. *A neurosis did not follow on this failure of differentiation.* After this, the animal was subjected to a series of tests lasting 2 hr. daily in which the whole range of rates was presented in turn, 120 beats being followed by shock, the others not. After a few weeks the defensive reactions to 120 beats were found to be greatly weakened, but after further presentations of 120 beats followed by shock violent defence reactions developed to *all* the tempos. These reactions persisted for at least 3 years. Since they followed a single-stimulus phase of the experiment, they were certainly not due to the difficult discrimination providing an ambivalent stimulus situation.

Rhythmic alternation of positive- and negative-conditioned stimuli. A sheep ((2), p. 120) was subjected to the following schedule from the beginning of its training: pure tone of 435 cycles; shock; 7 min. pause; pure tone of 900 cycles; no shock; 7 min. pause. The whole cycle would be gone through twice at each session. At the seventh observational

period, 26 days from the inception of the schedule, the animal resisted being brought to the laboratory, showed restlessness on the experimental table, and struggled vigorously when either tone was presented, a state of affairs which persisted for the rest of the animal's life. The only comment that need be made here is that the 7 min. interval seems too long to make this an ambivalent stimulus situation.

Increasing the daily number of reinforcements. In the case of the sheep reported by Liddell & Bayne (25) shock was administered to the foreleg at the sixth beat of a metronome at 60 beats per minute. Fifteen trials were given daily. After 98 trials a delayed conditioned flexion response was obtained. The number of trials was then increased to 20 per day. On the second day on which this was done, the animal for the first time resisted being led to the experimental room. Throughout the session the sheep was in almost constant movement. The delayed reflex was no longer obtained: instead, there was an exaggerated leg movement with each beat of the metronome. Continuing to experiment on the sheep led to increasing nervousness, even when tests were reduced to five per day.

Giving a large number of unreinforced presentations of a generalized stimulus. In Anderson & Liddell's 'Sheep 2' a very stable conditioned response was formed after 120 combinations of a metronome at 120 beats a minute and shock. A metronome beating 50 times to the minute was now repeatedly presented without shock. The leg-raising response became weaker but did not disappear. At the end of a series of 42 presentations distributed over 2 days neurotic symptoms appeared. It does not seem reasonable to argue that the neurotic effects were due to a clash between excitation due to generalization and the developing inhibition that is the essence of the process of extinction ((18), p. 281; (49)). If this *were* the case, it would also occur during extinction of conditionings unrelated to shock; but disturbances do not seem to have been noted in Pavlov's numerous experiments on extinction, also on space-restricted animals.

A different factor is given causal prominence by the Cornell experimenters for each of the above-mentioned procedures. Yet there is *a common factor* whose character they persistently regard as a matter of indifference—the repeated evocation of the leg-flexion response, either by the noxious stimulus (shock) or by the conditioned stimulus, or by a stimulus whose effects depend upon its similarity to the latter (generalization). Now, with the leg-flexion, the noxious stimulus evokes a certain amount of emotional disturbance in the animals, as evidenced in particular by Anderson & Parmenter's detailed case histories (2). This disturbance, too, would be conditioned to the conditioned stimulus. How an anxiety reaction of great strength can be built up on the basis of this will be discussed at a later juncture. The point being made here is merely that the origin of the neuroses should be sought in what is common to all the procedures—noxious stimulation in conditions of confinement.

Strong support is given to this point of view by certain unpublished Cornell experiments quoted by Anderson & Parmenter. Goldman working with goats, Jensen working with sheep, and Daniel and Anderson working with dogs are stated to have produced experimental neuroses by *rhythmic stimulation with positive stimuli alone.* Anderson & Parmenter agree with Liddell (24) in supposing *monotony* to be the operative traumatic factor here. But is monotony ever traumatic if no noxious stimulus is used? And would neurosis not result with positive stimuli alone even if they were not presented rhythmically? Liddell & Bayne's experiment (25) shows that it would. It is evidently easier to produce neurosis when rhythmic stimulation is used but this seems to be associated with the fact,

observed by Anderson & Parmenter themselves, that the waiting time-interval itself becomes an additional conditioned stimulus to anxiety responses.

Another series of neuroses alleged to be due to a clash between excitation and inhibition was produced by Cook (6) in movement-restricted rats. These were first trained to flex the right leg for a food reward in response to a bright light. If an animal raised its leg without a light appearing it would be lightly shocked. The leg-flexion habit having been strengthened over several weeks, the 'inhibitory stimulus', the shining of a dim light, was introduced. A flexion to this light brought a 'punishing shock'. If no flexion occurred the dim light would be extinguished after 5 sec. After the differentiation had been strengthened over several periods, the final stage of the experiment was reached—gradually increasing the intensity of the dim light. There were two stages at which special stress seemed to be imposed on the rats, *and both occurred before the discrimination became difficult*. The first was when leg-flexions unaccompanied by bright light were unrewarded and later punished, the second when leg-flexions to the dim light were unrewarded and punished. Cook's explanation is that during these stages there was a clash between excitatory and inhibitory tendencies to leg-flexion. Whether or not such a clash played any part could only be decided by a control experiment in which introduction of the shock would be without relation to the conditioned stimulus. But here it may be noted that in the circle-and-ellipse type of experiment of Pavlov no disturbance is mentioned concerning the stage when the first ellipse is being made a negative stimulus, even though here too there is a positive tendency due to generalization from the circle together with a negative tendency being built up by non-reinforcement of the ellipse. The difference may well be entirely attributable to the fact that Cook was using shocks at such a stage, while Pavlov was not. There is no doubt of the disturbing action of electric shocks; and no reason to suppose that a 'clash' added in any way to the disturbance.

The neuroses produced by Pavlov ((36), pp. 289–90) and Petrova (38) that come under the present heading are in somewhat different case. The animal was, as usual, held in position by a harness. Weak electrical stimulation of a spot on its skin was conditioned to the alimentary reaction by being repeatedly followed by food. The current was then gradually increased in strength from one experimental session to the next until it was 'extremely powerful'. The alimentary response continued to be evoked by this stimulus and remained stable for many months. There was never any sign of a defence reaction. Then the electric current began to be systematically applied to a different place daily. For some time the alimentary response continued to be evoked, but, on applying the current to a still further new place, 'a most violent defence reaction' occurred. No trace of the alimentary reaction remained. In another dog, when the explosive reaction had not yet been brought about by application of the electrical stimulus to a thirteenth new place, it was produced by applying the stimulus to several of the places on the same day, and not, as before, to a single place only. Pavlov states that because of the special nature of the conditioned stimulus little attention was given to these results at first (presumably because they seemed to be what one would expect of a severe noxious stimulus). Later, when the similar outcome of the circle-and-ellipse experiment had been observed, it was concluded that in both types of case the disturbances were manifestations of a 'pathological state' resulting from a clash between excitation and inhibition beyond a stage at which a balance could be maintained ((36), pp. 292–3).

However, the circle-and-ellipse and the shock experiment differ from each other

fundamentally. In the former, the 9:8 ellipse repeatedly evoked simultaneous conflicting tendencies to make and not to make the alimentary response. In the latter, until the final phase, the shock evoked only the alimentary response, because conditioned inhibition of the defence reaction had been gradually built up on the basis of reciprocal inhibition (49); and in the final phase the defence reaction appeared, alone and unrestrained. The final phase only occurred when the shock was applied in a way that went beyond the scope of the conditioned inhibition. It is quite possible that the same or similar effects would have been produced if the animal had merely been given a shock of the final severity on his first visit to the experimental table. The writer has accomplished such results in cats (see below).

(*b*) *Noxious stimuli interrupting conditioned feeding responses*

Dimmick, Ludlow & Whiteman (8) were the first to produce animal neuroses by a method that falls under this heading, although they attributed their results to difficult discrimination—mistakenly, as will be shown. Using an experimental cage $36 \times 28 \times 16$ in. they trained cats to raise the lid of a food-box in response to a lamp switched on simultaneously with the ringing of a bell. When 100 % correct responses had been acquired the cat would be shocked through a grid on the floor of the cage if he opened the food-box at any time except while the conditioned stimuli lasted or 6 sec. thereafter. (This shocking, of course, constituted an interruption of the feeding response conditioned, incidentally, to such stimuli as the sight of the food-box.) A varying number of sessions after this was begun, it was found that the cat would stretch towards the food-box at the conditioned stimuli but not touch it. When this happened the animal would be shocked at the end of the 'correct' period for opening the food-box. The effect of this was to produce progressively more negative behaviour towards the experimental situation, and such symptoms as yowling, crouching, and clawing at the sides of the cage developed as responses to the conditioned signals. Also, the cats became less friendly in their living quarters.

Masserman (29) has reported the production of numerous neuroses in cats by methods in many ways similar to the above. Using a cage $40 \times 20 \times 20$ in. in size, he also trained the animals to open the lid of a food-box in response to a light-and-bell signal. When this was well established he produced neuroses as follows. At the feeding signal, or at the moment of food-taking, a blast of air would be applied across the food-box, or the grid on the floor of the cage would be charged with two or three high-voltage, low-amperage pulsations. When the air-blast was used, the characteristic reaction was a sudden rush to crouch at the far side of the cage. The usual reaction to a grid-shock was 'a startled jump as each impulse was felt, followed by a slow dignified stalking away from the food after the shocks ended'. Sometimes the air-blast and shock were given together, and then the reaction usually resembled that to the shock alone. Whatever the noxious stimulation used (and the air-blast seems to have been disturbing enough to deserve the adjective) neurotic behaviour would afterwards be observed in the experimental cage—agitation or immobility, vocalization, refusal of food, sensitivity to extraneous stimuli, and, on presentation of the feeding signal, crouching, trembling, mydriasis, and other signs of anxiety. Outside the experimental cage some animals became excessively timid, while others become aggressive towards their cage-mates or towards humans.

Dimmick *et al.* (8) believed that their animal neuroses were due to 'difficult discrimination', and Masserman thought that his were caused by 'motivational conflict', that is,

a clash between a feeding motive and an avoidance motive created by the air-blast or shock. If these experimenters had made control observations as to the effects of shock on animals that had never been fed in the experimental cage they would not have come to such conclusions. The present writer has shown that a cat shocked in this kind of apparatus develops much the same changes of behaviour whether it has feeding motives at the time of the shock or not.

(*c*) *Noxious stimuli used alone*

The only previous report of neurosis production conforming to this heading appears to be that of Watson & Rayner (42). They found that a usually phlegmatic 11-month-old infant called Albert would react fearfully to the sound of an iron bar loudly struck behind his head. They contrived to strike the bar just as the child's hand touched a white rat. This was done 7 times and then repeated after a week. It was subsequently found that the child reacted with fear to the white rat, and also to a rabbit, a beaver fur, and a dog. A month later these fear reactions could still be elicited. After this the child was lost sight of.

(iii) *Rejection of Maier's 'abnormal behaviour fixations' as neuroses*

Maier (26) has reported a large number of experiments on rats, producing what he calls 'abnormal behaviour fixations' which he regards as parallel to other experimental neuroses. Rats were trained to jump to the cards on a Lashley jumping apparatus. During a 'correct' jump, the card, being unlatched, would be knocked over, and the rat would land on a feeding platform to receive a food reward. The 'incorrect' card would be latched, so that an animal jumping to it would be bumped on the nose and fall on to a net below. If jumping to one of the two patterns or one of the two sides was consistently rewarded, the animal would learn to jump to the 'correct' card. If, however, the cards were latched in no regular order, the animal would show a stage of variability in its choices and soon after refuse to jump. It would then be forced to jump by the administration of an electric shock at the jumping stand, prodding with a stick, or blowing a blast of air on to it. Maier noted that the jumping so compelled occurred consistently to one side (position stereotype) or to one pattern (symbol stereotype), even if the animal had his nose bumped at each jump. This unchanging habit is what Maier means by 'abnormal behaviour fixation'. He regards it as abnormal because it is persistent although repeatedly punished by nose-bumps, *and he believes, unrewarded.* But he overlooks the fact that each time a jump is *forced*, e.g. by electric shock, *its occurrence ends the shock.* This means a reduction of the drive that impelled the jump; and a drive reduction brought about in this manner is no less effective than a drive reduction due to reward for reinforcing stimulus-response connexions ((18), pp. 68–80; (45)). Jumping in the direction taken by the rat at the first forced jump will thus be reinforced, and reinforcement will be repeated at each forcing, with the result that jumping in this direction will be firmly established as the response to the electric shock stimulus. Clearly, this is nothing but ordinary adaptive learning, and there is no case for calling it 'an abnormal fixation'. Being adaptive, it is not neurotic behaviour in terms of our definition. Regarding the effects of the nose-bumping as a punishment, it should be pointed out that punishment has been found remarkably ineffective as a response-eliminating agent ((12); (40), p. 58). A more detailed criticism of these experiments is presented elsewhere (51).

Unadaptiveness could, of course, be claimed for the seizures displayed by some of Maier's animals in other experiments, but these are evidently due to an innate hypersensitivity to strong stimuli ((15); (26), p. 134). Maier ((26), pp. 135–9) disputes this on the ground that such seizures occur more easily in confined space (27) or when an animal is forced to jump to a card towards which it has an avoidance attitude (28); but this argument is untenable, since reactions dependent upon innate organization are also influenced by contingent circumstances. Unadaptive behaviour that is innate falls outside the scope of our definition of neurosis.

III. Present Experiments in Neurosis Production

These experiments were performed between June 1947 and July 1948 in the Department of Pharmacology of the University of the Witwatersrand. The present account is an abbreviated version of a more detailed one written in 1948 (43).

(i) *Subjects and apparatus*

The subjects were twelve domestic cats ranging in age from about 6 months to 3 years. They were housed in large, airy cages built into a brick cage-house on the roof of the department. When experimentation began an animal would have been in these quarters for at least 4 weeks. Experiments were always done between 11 a.m. and 1 p.m., when the animals had not eaten for 19–21 hr., except, occasionally, when prolonged observations were necessary.

The experimental centre was a laboratory on the floor below the cage house. Animals were carried to and from the laboratory in 'carrier cages' of which the dimensions were $9 \times 9 \times 16$ in.

The experimental cage ($40 \times 20 \times 20$ in.) was practically identical with that of Masserman, except that the long sides and roof were made of stout $\frac{3}{4}$ in. wire-netting in the place of glass. A metal funnel delivered pellets of minced beef into the food-box as required. The conditioned signals used were auditory—a buzzer, and a whirring sound made by the armature of an automobile hooter (inaccurately referred to as a 'hoot'). The grid on the floor of the cage could be charged by depressing a telegraph key in the secondary circuit of an induction coil continuously vibrating 25 ft. away. The current delivered, being of high voltage but low amperage, was very uncomfortable to the human hand but not productive of tissue damage.

In the course of the experiments it became evident that the experimental laboratory, and, indeed, the experimenter himself, could well be regarded as part of the apparatus. For convenience the experimental laboratory was called room A. Experiments were also conducted in three other rooms which were labelled respectively B, C and D. These rooms, in the order stated, seemed, to the human eye and ear, to have decreasing degrees of resemblance to room A. Rooms A and B were both situated about 30 ft. above ground-level overlooking eastwards a fairly busy street, but room A was the brighter of the two, as it also had windows on its north side. Both rooms contained very dark laboratory furniture, the greater quantity being in room A. Room C faced south, was about half the size of A or B, and contained laboratory furniture lighter in colour and less in quantity than that in B. It was out of earshot of the street. Room D was situated on the roof of the Medical School, and was extremely bright with white-washed walls and large windows

north and west. Besides a concrete trough and odd packages it contained only a light-coloured kitchen sink in one corner.

(ii) *Procedure*

Experimentation was commenced only after an animal's original reactions had been recorded to the experimental cage, to the auditory signals and various other sounds, and to one or more of the rooms B, C and D. In all cases these reactions were found to be inconsequential.

Six experimental animals were subjected to the main experimental procedure (schedule I) and six to a control procedure (schedule II). In brief, it may be said that schedule II corresponds to the method for producing neuroses described by Masserman (29), and schedule I differs from it in omitting what Masserman would regard as an essential step—the conditioning of a feeding response.

Schedule I. Not more than 2 days after the control observations detailed above, the cat was reintroduced into the experimental cage and given five to ten grid-shocks, each immediately preceded by a 'hoot' lasting 2 to 3 sec. The grid shocks were separated by irregular intervals ranging from 15 sec. to 2 min. With the exception of cat 8 who received five shocks in all, these animals were subjected to a second series of shocks 1 to 3 days later.

Schedule II. The first part of this schedule consisted of conditioning animals to perform food-approach responses to the buzzer. There were two variations differing from each other in a minor respect. Four of the six animals were trained to raise the lid of the food-box to the stimulus, the other two to orientate themselves to the *open* food-box. When either response had been strongly reinforced over eight to sixteen experimental sessions each consisting of about twenty reinforcements, each animal would be subjected to shocks from the floor of the cage, as follows. The buzzer had just sounded, the cat had made its learned response, the pellet had fallen into the food-box, and now the animal was moving forward to seize it. Before the pellet could be reached a grid-shock was passed into the cat which immediately recoiled, howling. Shocking under these conditions was repeated until the cat ceased to make its previous conditioned response to the buzzer. Two or three shocks were usually sufficient to accomplish this, but one cat required nine shocks distributed over three separate sessions.

(iii) *Immediate effects of the shock*

The immediate responses to shock followed the same pattern in all cats, whether they had previously acquired a feeding response in the experimental situation or not. This pattern was made up of various combinations of the following symptoms—rushing hither and thither, getting up on the hind-legs, clawing at the floor, roof and sides of the experimental cage, crouching, trembling, howling, spitting; mydriasis, rapid respiration, pilo-erection, and, in some cases, urination or defecation.

(iv) *Lasting effects of the shock*

(*a*) *Effects noted in the experimental cage*

When tested at subsequent sessions, *all* animals, irrespective of the schedule employed, displayed neurotic symptoms in the sense of the definition given at the beginning of this paper. The effects of the two schedules were broadly the same, but a few differences will

be mentioned below. Three manifestations were constant and common to all animals: (1) resistance to being put into the experimental cage; (2) signs of anxiety when inside the cage (muscular tension and mydriasis were invariable); (3) *refusal to eat meat pellets anywhere in the cage even after* 1, 2 *or* 3 *days' starvation.*

Quantitative change in general activity was almost invariable. An increase or decrease was usually constant for an individual animal. Increased activity took the forms of restless roving, clawing at the wire-netting, butting the roof with the head, and ceaseless vocalizing. Decreased activity varied between tense infrequent movements in the standing posture and very intense immobile crouching.

Symptoms that were observed intermittently in all animals were hypersensitivity to 'indifferent' stimuli, pilo-erection, howling, crouching, and rapid respiration.

Certain cats displayed special symptoms in addition to those that were common to all. The respiratory rate of cat 9 always rose from about 30 to about 60 as soon as he was put into the cage. Cat 15, who had micturated while being shocked, invariably micturated a few seconds after being placed in the cage. Cat 6 manifested almost continuous trembling. Cat 8 developed a symptom that it seems permissible to call hysterical. He jerked his shoulders strongly every few seconds in the experimental cage, and also in his living cage *if the experimenter entered it.* This jerking suggested an abortive jumping movement, and may well have had its origin in the fact that on the first occasion on which this animal was shocked he jumped through a hatch in the roof of the cage that had been left open inadvertently. It is worth noting that all but the first-mentioned of these four cats had been through schedule I.

Whatever symptoms an animal showed in the experimental cage were invariably *intensified by presentation of the auditory stimulus* that had been contiguous with the occurrence of the shock.

(*b*) *Generalization of neurotic responses outside experimental cage*

All cats showed some symptoms outside the experimental cage of the same kind as inside it. In two schedule I animals and four schedule II animals these effects were limited to slight tenseness and fluctuating mydriasis in the experimental room, together with inhibition of feeding responses anywhere in the room at the sound of the buzzer or 'hooter' as the case might be. However, in the remaining six cats (four of schedule I, and two of schedule II) such symptoms were very marked, and were observed also outside the confines of the experimental laboratory. That is to say, they were observed in one or more of the rooms B, C and D. One animal also refused meat pellets in the passage in front of the living cages. Any animal that showed symptoms of tension in room D showed them more strongly in room C, and still more in room B. A cat free of symptoms in room B would also be free in rooms C and D. Thus it would appear that the anxiety-producing effect of these various rooms was a function of their resemblance to room A as judged in a rough way by the human eye and ear. More exactly, the magnitude of the anxiety response in room C, for instance, seems to have depended upon the number of stimuli common to rooms A and C. As will be seen below, the successful use of these rooms to build up a conditioned inhibition of the anxiety responses provides strong support for this supposition.

(*c*) *Neurotic responses to the experimenter*

Three schedule I animals developed phobic reactions towards the experimenter which were manifested even in the living cages. The jerking shoulders of cat 8 when the experimenter entered his cage have already been mentioned. Two others would crouch with pupils widely dilated as soon as they saw even his entry into the cage-house. Such effects were never noted in schedule II animals.

(*d*) *A note on some differences between the effects of the two schedules*

The general similarity between the effects of the two schedules has been mentioned. However, it is only to be expected that the preliminary conditioning of feeding reactions in schedule II animals would result in some differences. The fact that only schedule I animals became phobic towards the experimenter was noted in the last paragraph. The explanation seems to be that for schedule II cats 'approach' attitudes towards the experimenter had at first been built up as a result of his previous association with feeding responses; and at the time of shocking, the experimenter was relatively remote as a stimulus since the animal was usually almost continuously orientated towards the food-box. Schedule II animals were alone found to show phobic responses to meat pellets dropped in front of them wherever they might be—starting with pupils widely dilated. This is easily understood, as these animals were responding strongly to the sight and smell of these pellets when they were shocked.

IV. Curative measures involving reciprocal inhibition

The fact that the neurotic reactions of the cats were associated with inhibition of feeding suggested that under different conditions feeding might inhibit the neurotic reactions: in other words, that the two reactions might be reciprocally inhibitory.

In order actively to inhibit the anxiety reactions the feeding would have to occur in the presence of anxiety-producing stimuli. Either, then, some factor that would favour feeding had to be added to the stimulus situation in the experimental cage, or else feeding had to be attempted somewhere *outside* the experimental cage where anxiety-producing stimuli would be less numerous and less potent. Both principles were used, as described below.

(i) *The addition to the experimental environment of a factor favouring feeding*

(*a*) *The human hand as a stimulus to feeding*

Since in their living cages the cats were accustomed to have food cast to them by the human hand, it was expected that the hand had become conditioned to evoke approach responses to food. Consequently, the experiment was tried of placing an animal in the experimental cage and moving towards its snout pellets of meat on the flat end of a 4 in. rod held in the experimenter's hand, in the hope that the presence of the hand would overcome the inhibition to eating. This procedure was applied to nine animals (six schedule I, and three schedule II), and after some persistence four of them (three schedule I, and one schedule II) were induced to eat. These animals at first approached a pellet hesitantly, sometimes refusing it. But after several had been eaten, they ate fairly freely from the rod, and soon after would also now and then eat a pellet on the floor of the cage or in the food-box. In the case of cat 3, the only one of these four cats that had been

through schedule II (in which shock interrupted a movement towards the food-box), eating out of the food-box did not occur until he had eaten more than fifty pellets on the floor of the cage in the course of several sessions. As the number of pellets eaten in the situation increased, each of these animals ate more freely, moved about the cage with greater freedom, and showed decreasing anxiety symptoms. By the time cat 10 had eaten about fifty pellets and cat 11 about eighty they were jumping spontaneously out of the carrier into the experimental cage; and inside the cage were showing no sign of anxiety whatever. Cat 8, who, it will be remembered, had the 'hysterical' jerking movement of the shoulders, lost all trace of his symptoms only after he had eaten 216 pellets in the experimental cage in the course of eleven sessions.

(*b*) *Masserman's 'forced solution'*

The three schedule II cats on which the human-hand technique was not tried were each subjected to procedures based on the 'forced solution' described by Masserman ((29), p. 75), with like results. The essence of this method is that a hungry neurotic cat in the experimental cage is gradually pushed by means of a movable barrier towards the open food-box which contains appetizing food. This accentuates the manifestations of anxiety, but after a while the animal snatches at the food in one or more hurried gulps. Repeating the procedure over several days has the effect of diminishing and then eliminating the neurotic reactions.

(ii) *Feeding in the presence of relatively weak anxiety responses*

It has already been stated that the inhibition of eating, together with the rest of the neurotic picture, occurred not only in the experimental cage but also anywhere in the experimental room and in other rooms resembling this room in various ways—an effect presumably due to primary stimulus generalization ((18), p. 184; (47)). The slighter the resemblance of a given room to the experimental room the less marked were the anxiety reactions. In the case of the five cats who had remained unaffected by the delivery of food in the experimental cage by the human hand, it was decided to try to obtain feeding responses in the rooms where the anxiety reactions were less marked. It was thought that somewhere in this 'hierarchy' of rooms there would be found for each animal a place where the anxiety responses were mild enough to permit eating at least at times. Starting in the experimental room (room A), each cat was patiently plied with meat pellets on the floor. If it did not eat after about 30 min. the experiment was repeated on the following day in the next room lower in the 'hierarchy'. One cat ate initially in room A, one in room B, one in room C, and one in room D. The fifth animal could not be persuaded to eat even in room D, but did so eventually in the passage that separated room D from the living cages. Once an animal had eaten in a given place it was given about twenty pellets there, always responding to their presentation with increasing rapidity, and with decreasing signs of anxiety. The next day it was tested in the room next in order of resemblance to room A. (From time to time control tests of response in the experimental cage were also made.) By this method of gradual ascent all the animals were eventually enabled to eat in room A. Then, within the room, gradual approach was made to the experimental cage in similar fashion, the animal being fed on the floor increasingly close to the experimental table, then on the table next to the cage, on the roof of the cage, and at last inside the cage. It was found that when feeding became possible in the cage

anxiety reactions were much more rapidly eliminated there when the pellets were tosse at widely distributed points than if they were confined to the food-box. Apparently scattered placing of the pellets resulted in reciprocal inhibition of the anxiety responses t stimuli from all parts of the experimental situation, and this made possible the develop ment of conditioned inhibition of the responses to all parts (according to the mechanism discussed below), so that after 50–100 pellets had been eaten in the cage, manifestations o anxiety ceased to be observed.

(iii) *The elimination of neurotic responses to the conditioned auditory stimuli*

The fact that an animal, after subjection to the procedures just described, would behav without anxiety in the experimental cage can be attributed to the elimination of th neurotic responses that had previously been conditioned to the various visual (an olfactory) cues in the vicinity of the cage. But if the *auditory* stimulus that had precede the shocks was now presented the animals again manifested a high degree of anxiety. Th effect was such that the animal could be inhibited by the auditory stimulus from com pleting any movement towards a pellet of meat in the experimental cage, and, in mos cases, at various points in the experimental room as well. The problem was to present th auditory stimulus in such a way that the anxiety reactions would be weak and there woul be no inhibition of eating. There were two possible solutions to this: (1) diminishing th strength of the stimulus either by reducing its physical intensity or by increasing th distance between the animal and the stimulus; or (2) making use of the stimulus trac and the fact that the effects of a brief sensory stimulus on the nervous system gradually decline in intensity with passage of time(1). The first solution was applied to tw animals and the second to seven.

(*a*) *Feeding in conjunction with the auditory stimulus at reduced strength*

Both cats upon whom this procedure was used had been through schedule I. Th minimum distance at which cat 11 would eat with the conditioned auditory stimulu ('hooter') sounding continuously was found by trial and error to be 40 ft. Here, thoug continuously tense and mydriatic, the animal ate eight pellets, but would not eat a a point 10 ft. nearer. The next day, after two pellets at 40 ft., he ate ten at 30 ft., a first only when dropped a few inches away from him, but afterwards even at distances o 3 or 4 ft. Pellets were then dropped increasingly near the 'hooter', and by the time eigh more had been given he had eaten two only 17 ft. away. After eating each pellet he woul run back to the 30 ft. point, or even beyond it. Day by day, the distance at which th animal would eat was reduced. When he had had a total of 160 pellets at progressively decreasing distances he was at last able to eat inside the experimental cage during th sounding of the 'hooter', although manifesting considerable anxiety. At the end of hi fourth session spent in eating in the cage, by which time he had eaten a total of eighty seven pellets there, the note was made, 'There is now absolutely no sign of avoidanc reaction or anxiety to the auditory stimulus'.

Cat 8, whose anxiety responses to the auditory stimulus were much milder than thos of cat 11 at the commencement of this procedure ate as little as 20 ft. away from th 'hooter' in the beginning, and required only thirty-three pellets at decreasing distance to enable him to eat in the presence of the sound in the experimental cage. He lost al tenseness there after forty-five pellets given over four sessions.

(*b*) *Feeding in the presence of the trace of the conditioned auditory stimulus*

Four of the cats treated in this way had been through schedule I and the other three through schedule II. The animal was placed inside the experimental cage, and though showing no sign of disturbance was given one or two meat pellets on the floor of the cage. Then, after an interval of about a minute, the auditory stimulus was sounded for about one-fifth of a second, and *immediately afterwards* a pellet was dropped in front of the animal, who had in the meantime become tense and hunched-up, with pilo-erection and mydriasis. After a variable interval, usually about 30 sec., the animal would have lost most of its tenseness and would move forward cautiously to the pellet and eat it. Not less than 1 min. later, the auditory stimulus would again be presented, making the cat tense, and be followed by a meat pellet, but this time the animal would eat after a shorter interval. This procedure was repeated 10 to 20 times during a session, and the interval before the eating of the pellet would gradually diminish until the animal ate without delay. In illustration of the above, the intervals recorded (in seconds) for one cat during the first session of this kind were: 40, 15, 20, 7, 5, 6, 6, 6, 4, 8, 8, 11, 4, 6, 4, 4, 5, 3, and 4. At the next session the intervals were 3, 3, 2, 3, 2 and 2 sec. for the first six presentations of stimulus, and thereafter she consistently responded practically at once.

The next step was *to increase the duration of the auditory stimulus*. First, every fourth or fifth stimulus was given a duration of about a second; and then, gradually, depending on the responses of the animal, the duration was increased. Eventually, the animal would show no vestige of anxiety even to a stimulus of 30 sec. duration, and would make alert food-seeking movements as soon as she heard the auditory stimulus.

(*c*) *Demonstration that the neurotic reactions were not merely overshadowed*

Whichever of the above two procedures had been employed, the question remained whether the neurotic reactions had really been eliminated or had merely been overshadowed by a stronger reaction of feeding beneath which they lay dormant. The decisive experiment was *to extinguish the food-seeking response to the auditory stimulus* and then observe whether or not the neurotic reactions were reinstated. Each of the animals was given thirty irregularly massed extinction trials on each of three successive days. Long before the end of the third day's session they all showed almost complete indifference to the auditory stimulus. Immediately after the conclusion of the third extinction session the following test was made. A pellet was dropped on the floor of the experimental cage about 2 ft. away from the animal, and as he began to approach it the auditory signal was sounded continuously, to see if extinction had reinstated the inhibitory effect on eating that had originally been noted. *In no instance was there observed any semblance of the restoration of an anxiety response or any suggestion of an inhibition of eating.* Moreover, observation for many weeks afterwards never revealed recurrence of anxiety responses in any animal.

V. INTERPRETATIONS OF THE PRESENT RESULTS

(i) *Conformity of the behaviour changes to definition of experimental neurosis*

According to the definition given at the beginning of this paper an experimental neurosis has to satisfy criteria of anxiety, unadaptiveness and persistence. The prominence of anxiety is sufficiently obvious from the account of the experiments and need not be discussed further.

Unadaptiveness was shown in two ways—the inhibition of feeding responses i a hungry animal in a situation in which feeding would not again be followed by shoc (and in the case of schedule I animals had never been followed by shock); and the mer fact that the anxious responses were causing fatigue without in any way favouring th organism's life processes (i.e. were without reward).

The criterion of persistence was satisfied by: (1) maintenance of tenseness and othe manifestations at an undiminishing level throughout a long session; (2) recurrence c manifestations at unabated strength session after session; and (3) re-evocation of mani festations after long periods free from experimentation, for example, in all of three cat rested for 6 months.

The following extract from the protocols of cat 7 (schedule I) illustrates the unremit tingness of the neurotic tension and shows how complete the inhibition of feeding could be

On September 1, 1947, seven days after she had been shocked, cat 7 was taken when hungry to th experimental laboratory. She resisted being put into the experimental cage. Inside the cage, she crouche with pupils widely dilated. She was kept there for 2 hours, during which her pupils remained dilated although their size fluctuated a good deal, reaching a maximum at any sharp sound or sudden movement At times she became very restless, mewed plaintively, and clawed at the sides of the cage. She showed n response at all to meat pellets put in front of her nose on an ebony rod, and left untouched a pelle which lay on the floor of the cage during the whole of her two hours' confinement.

(ii) *Why the behaviour changes are regarded as learned*

In order to decide whether or not the neurotic behaviour was learned it is necessar first to define what is to be understood by the term *learning*. Learning may be said t have occurred if a response has been evoked in temporal contiguity with a given sensor stimulus, and it is subsequently found that the stimulus can evoke the response althoug it could not have done so before. If the stimulus could have evoked the response befor but subsequently evokes it more strongly, then, too, learning may be said to hav occurred.

In the case of our experimental cats there was no qualitative difference between th immediate responses to the shock and the later responses to the experimental environment In other words, the responses produced by the shock in the originally 'neutral' experi mental environment were subsequently found to be producible by the experimenta environment itself. This clearly implies the occurrence of learning as defined above. I was particularly well shown in the development of the 'special' symptoms noted earlie For example, the cat that displayed the jerking shoulders was the one that had onc jumped out of the cage when shocked, and the one that regularly micturated in th experimental cage had micturated when shocked.

Furthermore, it will be seen below that these experiments embody other features tha make the results easily explicable in terms of modern knowledge of the learning proces

(iii) *The present results in the light of modern learning theory*

(*a*) *Preliminary remarks on modern learning theory*

By modern learning theory is meant the recently developed body of psychologic theory associated largely with the name of C. L. Hull (18). Involving a rigorous scientif discipline and placing much emphasis on quantification, this body of theory has gone f towards raising the methodological level of psychology to that of the older science

To-day it increasingly dominates the literature of American experimental and theoretical psychology.

A few of the basic notions of modern learning theory must be briefly mentioned. They derive essentially from Hull's classic work (18), but are modified in certain respects out of consideration of recent neurophysiological knowledge (44,45,47,48,49).

In 1938, Culler (7) showed in dogs that when a semitendinosus response was established to a tone, stimulation of a point found by trial and error on the exposed cortex also evoked the response, and ceased to do so if the response to the tone was extinguished. This finding strongly supports the supposition that learning is subserved by the development of conductivity between neurones in anatomical apposition. Numerous experiments have demonstrated that in a given learning situation there is a direct quantitative relationship between the amount of learning that occurs and the magnitude of the associated drive reduction. It has been argued elsewhere (45) that if learning has a neural basis, then it is through reduction of a feature common to all drives—*central neural excitation*—that drive reduction subserves learning. The strength of central neural excitation can be appraised indirectly by knowledge of stimulus strength or of response strength. (In the case of human subjects, some indication of strength of drive can, of course, be obtained from reports of experience.)

The process of *elimination* of learned reactions is conceived to involve the weakening of neural connexions previously formed during learning. The process occurs in either of two ways—extinction or reciprocal inhibition, and in both of them drive reduction plays as important a part as in learning. In the case of *extinction* the drive concerned is the fatigue-associated state of disequilibrium having its origin in the effector organs subserving the response—the Mowrer-Miller hypothesis ((18), p. 277; (32),(35)). *Reciprocal inhibition* implies that in the presence of stimuli that would lead to the given response, another, incompatible and stronger response is enabled to occur, and the given response is inhibited (49). In such cases, an obvious drive reduction is always provided by the reciprocal inhibition of the excitation that would have led to the given response; and if the response that was dominant is rewarded its drive is also reduced.

(*b*) *The learning process in the present experiments*

Reasons were given above for deciding that the neurotic reactions of our cats were learned. The high intensity of the reactions after very few shocks can be accounted for on the basis that a very powerful drive reduction occurred *at the cessation of each shock.* The responses occurring at the time of the cessation became strongly connected thereby to stimuli from the experimental environment. These responses comprised musculoskeletal movements tending away from the situation and autonomic responses evidenced, for example, by muscular tenseness, mydriasis, and pilo-erection. The whole picture was one of 'anxiety'.

After the first shocking, stimuli from the experimental environment were already able to elicit some measure of anxiety responses, and these, of course, had an anxiety-drive state as an antecedent. In other words, the environmental stimuli had become able to arouse a secondary (i.e. learned) drive state. Reduction of this secondary drive by the removal of the animal from the experimental environment would further reinforce the anxiety responses as responses to the stimuli of that environment. O. H. Mowrer (33,34)

was the first to recognize the role of anxiety reduction as a reinforcing agent. Othe workers who have demonstrated this are Farber (13), May (30), and Miller (31).

(*c*) *The factor of confinement*

Anderson & Liddell (1) have pointed out that all animal neuroses have been produced i conditions of confined space. Cook's experiments (6), employing different degrees of spac constriction with other factors constant, indicate that it is a potent factor in neurosi production. There are at least three ways in which confinement could exert its influence First, the prevention of an effective escape response allows the cumulative action on th animal of stimuli that have become conditioned to anxiety, so that there is a risin magnitude of potentially reducible anxiety drive. Secondly, the drive reductions occu in the presence of a limited number of stimuli, and the anxiety responses can be con ditioned to these in greater strength than would be the case if the environment wer a changing one. Thirdly, it seems likely that autonomic responses are stronger when free from the reciprocally inhibiting effects of musculo-skeletal responses (51).

(*d*) *The reason for persistence of the neurotic responses* (*resistance to extinction*)

As stated under (*a*), learned responses that are unrewarded (and so unadaptive ordinarily undergo extinction by a mechanism that is associated with fatigue. Sinc fatigue occurs whether there is reward or not, there is *always* a fatigue-associated driv state that tends towards a weakening of the connexion between the response and th stimulus that led to it. But it is only when there is no significant reward that the con nexion is *actually* weakened((18), p. 298). When reward counteracts this tendency toward weakening it does so through reducing the central drive state due to, say, hunger.

Now, the neurotic responses are unrewarded (unadaptive). Yet they necessarily hav a central state of drive as an antecedent. This drive is automatically reduced by th retreat of the organism from the anxiety-producing stimulus, or by any other mean that removes the organism from the action of this stimulus. Any responses that may b occurring at such a time will be reinforced, and their fatigue-associated tendency to b extinguished will be counteracted. Farber (13) has presented a clear demonstration i rats of the manner in which anxiety drive reduction interferes with extinction.

Frequently repeated or continuous consequents of the anxiety drive (of which th autonomic responses are an example) are much more likely to coincide in time with driv reductions than are responses that occur only occasionally. That is why, in our exper ments, the autonomic responses, whose evocation was continuous, were always persisten whereas such intermittent responses as clawing the sides of the cage were soon extinguishe It is important to note that these autonomic responses were reinforced even though the did not themselves bring about removal from the shock.

(*e*) *The mechanism of cure*

The effectiveness of the procedures that overcame the neurotic responses can b accounted for as follows: in every instance feeding was made possible in the presence stimuli conditioned to anxiety responses which, under other circumstances, inhibit feeding. When stimuli to incompatible responses are present simultaneously the occurren of the response that is dominant in the circumstances involves the reciprocal inhibitio of the other. As the number of feedings increased, the anxiety responses gradual

became weaker, so that to stimuli to which there was initially a response of the anxiety pattern there was finally a feeding response with inhibition of anxiety. This 'permanent' change implies a new positive conditioning *pari passu* with a conditioning of inhibition, and this could have been subserved by at least two drive reductions—hunger drive reduction, and reduction by the reciprocal inhibition itself of the drive antecedent to the anxiety responses. That the conditioned inhibition was due to a *neural change* and was not merely a correlate of the occurrence of the alternative response seems a clear inference from the fact that eventual extinction of the alternative response did not result in reinstatement of the original one. This mechanism has been more fully discussed elsewhere (49).

At this point it is necessary to comment on Masserman's interpretation of the cures obtained by the 'forced solution'. He states ((29), p. 203) that with the cat maximally hungry and the food very attractive, at a certain proximity to the food, the food-approach drive reached such an intensity as to break through the animal's inhibitions; and 'once the motivational impasse was disrupted, the feeding behaviour soon became more natural...and the other neurotic manifestations rapidly diminished in intensity'. Now, it can scarcely be doubted that the creation of a very intense drive to feed makes it possible to overcome the inhibitory effects of the anxiety state at a given moment. But it is still necessary to explain how the feeding results in a 'permanent' decrease in anxiety. It is by no means self-evident that the momentary overcoming of an inhibition implies the breaking of a *habit* of inhibition, as Masserman seems to think.

VI. CRITICISM OF PREVIOUS THEORIES OF EXPERIMENTAL NEUROSIS

(i) *Pavlovian theory and its modifications*

Pavlov's theory of the causation of neurosis follows from his general proposition that normal cortical function requires a balance between excitatory and inhibitory processes. The theory is essentially this ((36), pp. 292–3): if, at a given locus of the cortex, excitation and inhibition come into conflict at high intensity, the neural elements concerned may be unable to bear the strain, and may consequently be thrown into a pathological state.

The purport of this hypothesis depends entirely upon the meaning to be ascribed to the word 'pathological'. Pavlov does not mean merely that the clashing of excitation and inhibition are the necessary pre-conditions for establishing the neural connexions that subserve the neurotic behaviour, which would then be regarded as 'pathological' just because it is unadaptive. On the contrary, he implies that certain nerve cells are changed physically in some way that makes their functioning different from that of normal cells.

There is an extremely cogent reason for rejecting the idea of a physical pathology—the very fact that it is possible to eliminate neurotic responses by providing conditions which enable other responses to become reinforced to the anxiety-connected stimuli. If the neurotic responses can be unlearned they must presumably have been established in the first place by learning. A *lesion* in the nervous system could scarcely be expected to heal just because some specific new learning has occurred.

An interesting modification of Pavlov's theory was proposed by Anderson & Liddell (1). Realizing the importance of restraint in the production of experimental neuroses, they proposed the hypothesis that suppression of the animal's 'spontaneous activity' results

in an accumulation of nervous tension that may be too great for the nervous system t withstand. Their views, however, resemble those of Pavlov in that they too believe tha the undesirable effects of the tension are due to 'actual damage to the nervous system' Essentially the same views have been expressed more recently by Anderson & Parmenter (2 and by Liddell (24).

Most other writers who have dealt with experimental neuroses, for example, Dworki and his associates (9, 10), Cook (5), Babkin (3) and Ischlondsky (19), have accepted the inter pretations of Pavlov and of the Cornell workers, and have not offered any significan elaborations of the theory. The conception of 'physiological disruption' put forward b Hebb (17) is basically similar, although it is expressed in somewhat different terms.

(ii) *Fenichel's psycho-analytic theory*

Fenichel (14), taking the view that the experimental neuroses correspond to those huma neuroses that are due to conflict, has tried to explain the experimental neuroses in term of psycho-analytic principles. He says that in the neurosis-producing situation th animal feels contradictory impulses 'between an instinct striving for discharge and th defensive forces of the ego'; but the conflict makes it impossible for him to discharg the impulses in the usual way. As a result, tension accumulates, and the organism i subjected to an amount of excitation greater than it can master. The neurotic symptom are regarded as 'distorted discharges as a consequence of the state of damming up'.

Now, even if it were to be accepted that an organism may be overwhelmed at a give time by powerful excitations with results as envisaged by Fenichel, the 'permanence' o the neurotic reactions remains to be explained. The explanation suggested by Feniche is that the excessive excitation has left some kind of nervous damage behind. This is th same as Pavlov's theory which was criticized above.

(iii) *Masserman's 'biodynamic' theory*

This theory of experimental neurosis is also basically psycho-analytic in character However, for Masserman the conflict is not between the animal's id and ego, but betwee two or more motivations. He states ((29), p. 103): 'When in a given milieu two or mor motivations come into conflict in the sense that their accustomed consummatory pattern become incompatible, kinetic tension (anxiety) mounts and behaviour becomes hesitant vacillating, erratic, and poorly adaptive (neurotic) or excessively substitutive, symboli and regressive (psychotic).' Thus, when conflict produces neurosis it is because 'behaviou is disrupted'. Masserman does not even suggest by what mechanism the disruptin conflict leaves lasting effects on the behaviour of the animal. If a group of cells wer physically disrupted their subsequent behaviour might be expected to change. Bu 'disruption of behaviour' by conflict merely implies the interrupting and distorting effect of an activity that intrudes upon the ordinary course of another. The mere occurrenc of such interference is not in itself an explanation of subsequent lasting changes i behaviour.

VII. GENERAL APPLICATION OF THE LEARNING PROCESS TO EXPERIMENTAL NEUROSES

(i) *Experiments employing severe shock*

Our own experiments were performed in a way that showed up unmistakably the learned character of the 'permanent' behaviour changes. It is easy to see that the neuroses produced by the other workers who also used strongly disturbing electric shocks, i.e. Cook (6), Dimmick *et al.* (8), Masserman (29), and Pavlov (36), must also have been established by the learning process. It has already been pointed out that 'conflict' would never have been invoked as an explanation if these workers had tried, as a control, the effects of shock in the absence of previous food conditioning in the experimental situation.

(ii) *Experiments employing mild shock*

Mild shocks were employed by the Cornell investigators (see above, p. 248). Yet there was always some evidence of general disturbance, even if the animal took only 'a few seconds' to quiet down. This is consonant with the human experience that an electric shock strong enough to evoke a motor response will also cause some emotional disturbance. As might have been expected, the mild shocks had to be administered many times more than severe ones to produce a similar measure of 'permanent' change of response involving emotional disturbance.

It is necessary to explain how a severe anxiety reaction could arise from what is apparently nothing more than the repeated conditioning of a mildly disturbing emotional reaction. The explanation offered here is that when emotional conditioning first begins to be established the conditioned stimulus evokes a small amount of anxiety. Let us call this x, and let it be half the amount evoked by the shock ($2x$). At this time, when the shock follows shortly on the conditioned stimulus the anxiety from the two sources will add together, giving a total of $3x$. As the result of conditioning, the next presentation of the conditioned stimulus will evoke, say, $1\frac{1}{2}x$, and the shock that follows will bring the total to $3\frac{1}{2}x$. It may then be found that the next presentation of the conditioned stimulus evokes $2\frac{1}{4}x$, that is, more than the shock itself. By repetition, the amount of anxiety evocable by the conditioned stimulus will gradually rise to a magnitude many times $2x$. At some stage the anxiety will be great enough to produce obvious signs, and as experimenting continues these will be intensified. This is clearly shown in Anderson & Parmenter's detailed case histories (2).

It should be noted, regarding the Cornell experiments, that it was not only to the special auditory stimuli that anxiety responses were being conditioned, but to the whole experimental environment. In the 'rigid time schedule' experiments, conditioning occurred also to the internal changes associated with a particular time interval, as Anderson & Parmenter point out, and this may have accounted for the easier production of neurosis in these cases. It seems gratuitous to postulate 'monotony' as a self-sufficient factor in the way Liddell has recently done (24).

(iii) *Ambivalent stimulation experiments*

Where neuroses have been produced by ambivalent stimulus situations it is less obvious that the learning process is the responsible mechanism. But there are two considerations in favour of such a hypothesis. First, the symptomatology of these neuroses seems to be the same as those produced by the use of noxious stimuli. Secondly, it is

a matter of everyday experience that in human subjects ambivalent stimuli arouse feelings of tension. In the following paragraph a hypothesis is presented of the mechanism whereby the small degrees of tension ordinarily evoked in an ambivalent stimulus situation may, through the learning process, become the basis of a severe emotional disturbance.

In ambivalent stimulus situations a stimulus is presented which, because of the special character of previous conditionings, evokes equal tendencies to the performance at one and the same time of two responses that are incompatible with each other. However slight and transient the anxiety produced by this conflict of tendencies may be at first, the drive reduction associated with its cessation results in the anxiety responses becoming reinforced to whatever stimuli are contiguous, and among these is the ambivalent stimulation itself. In the same way as suggested above for the case of mild noxious stimuli, the strength of the anxiety responses is conceived to be gradually stepped up at each presentation of the ambivalent stimulus situation while the drive-reduction-potential correspondingly grows. Eventually very powerful anxiety responses are evoked, and strong avoidance behaviour entirely replaces the approach responses to the experimental situation which were previously established by repeated feeding.

VIII. Clinical implications

Since 1947 the writer has studied a large number of human cases of neurosis with a view to determining to what extent their causation parallels that of the experimental neuroses. The findings of these investigations can only be summarily mentioned here.

From the outset, the fact that in human case-histories, too, conflict or trauma or both are invariably found seemed to support the idea of a common basis; and the experience of the past 4 years has encouraged belief in the hypothesis that experimental and clinical neuroses are parallel phenomena. Some of the similarities, discussed at some length in a dissertation (43), have been briefly reported (46).

The human subject is not often forced to undergo his conflicts or his traumata in physically confined space. He is usually kept in the anxiety-producing situation by the force of habits previously learned. For instance, a woman entangled in a humiliating marriage may be unable to get out of it because her earlier training has given a horror to the idea of divorce. Besides confining her within the marriage, this feeling of horror, being in conflict with escape tendencies, makes possible the development of a high level of emotional tension (anxiety). This tension becomes increasingly conditioned to contiguous stimuli through the drive reductions that follow every partial escape from the causative situation.

In therapy, the writer has concentrated on seeking means of obtaining reciprocal inhibition of anxiety responses, after the pattern that was so successful in animals. In the great majority of cases reciprocal inhibition of anxiety has in fact been procurable, either in the interview situation or the life situation, always with results that are beneficial, sometimes in a most dramatic way. The methods used and their effects are discussed in detail elsewhere (50). In an earlier communication (46) reasons were given for supposing that the similar amounts of success obtained by therapies differing widely in character are largely due to something common to all interview situations—reciprocal inhibition of anxiety responses by more powerful antagonistic emotional responses evoked in the patient by the therapist and by the interview situation in general.

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METHODS OF STUDYING SMALL SOCIAL GROUPS

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I. *Introduction* (pp. 269–270). II. *The experimental method* (pp. 270–276): (i) *The design of small-group experiments* (pp. 270–272); (ii) *Generalization from the results* (pp. 272–274); (iii) *A brief survey of experimental work on small groups* (pp. 274–276). III. *Conclusions* (pp. 276–277). *References* (pp. 277–279).

I. Introduction

The small social group has become the focus of considerable interest on the part of social psychologists. There are perhaps three main reasons for this interest. In the first place social psychology has reshuffled its subject-matter so that it may be said to be the study of human interactions against a background of the culture and of the social groups within it. Newcomb's recent text-book (46) expresses this new orientation very clearly. A distinction should be drawn straight away between a *temporary group* of people drawn together for a morning's experimentation—the small-scale equivalent of a crowd—and a relatively permanent or *primary group* with its norms, status order and stable interaction pattern. Studies of temporary groups are really studies of individual social behaviour in the culture, while studies of primary groups are studies of social behaviour in a social system superimposed on the culture. The reason for interest in *small* groups is that here people react to, and form relations with, the others as specific individuals; in fact this characteristic will be the basis of the definition of *small group* to be given below. For the same reason, interest has moved from the study of groups of people attending primarily to laboratory tasks to situations like discussion groups where the members are attending primarily to one another.

In the second place, small social groups are of interest as a phenomenon in themselves. Sociologists like Elton Mayo (40, 41) and Shils (55) have pointed out the importance in society of small groups like work teams, army units and gangs, while committees and therapy groups are also of great practical importance. Consequently, many workers have been concerned with the group as a whole and with explanations at the 'group level of analysis' (Krech & Crutchfield (32), pp. 14 f.).

Finally, the small group may prove to be of some value as a small-scale replica of wider social processes. Several studies have been conducted with an eye towards explaining behaviour in larger groups. Mintz (44), for instance, produced an analogue of crowd behaviour, Kelley (31) of the class system and Festinger (19) of social control. While future sociologists may well turn first to such studies in order to see their problems in simple form, they will have to bear in mind that the generalization to groups of larger size may be invalid, and that new phenomena may emerge with increasing scale. Swanson (58) suggests that such generalization can be valid if the 'essential underlying dynamics' of the larger system can be abstracted. However, such abstraction could only be made in the light of particular theories which themselves await proof. Better, as Swanson also suggests, to make predictions from the small-group results and to confirm them in the larger context. In view of the difficulties of control, the confirmation of such predictions will not be so valuable as are the experimental results for the small group, but they are the best that can be done. For the whole point of studying a small group in place of a larger is that

it can be studied in greater detail and in particular by experimental techniques, so that variables can be manipulated and controlled and direct causal relations discovered. Section II of this paper sets out to describe the methods of experimental inquiry in this field and to survey the results obtained. (In a later paper it is hoped to do the same for the observational methods and also to deal with the details of recording those aspects of the social system that are chosen as dependent variables.)

II. The experimental method

There is at present no agreed definition of *small social group*, and it may be a mistake in the present state of theory to seek one. Instead, one can point to the growing tradition of studies of groups of less than, say, fifteen. It may be suggested, however, that the essential feature is that a number of people interact over a period of time and that each is aware of all the others. Bales, for instance, defines a small social group as 'any number of persons engaged in a single face-to-face meeting or a series of such meetings in which each member receives some impression of the others as a distinct person even though it was only to recall that the other was present' ((4), p. 33). Within this classification of *small* social groups should be distinguished *temporary* and *primary* small social groups as above.

Again it is difficult, though not so important, to get a definition of *experiment* in this connexion, for it has been variously used in the social sciences (25). It will be used here, as it is by Underwood ((64), pp. 11–14), for investigations in which the group or the situation is varied by the experimenter and controlled observations made, so that a definite hypothesis about the relation of variables is tested (a 'crucial' experiment), or the presence or absence of relation between variables studied (an 'exploratory' experiment). This excludes so-called 'natural' experiments and Chapin's *ex post facto* design (12), wherein experimental and control groups are selected after the event from social records and followed up in the present.

(i) *The design of small-group experiments*

There are three basic designs for psychological experiments. In the first, matched or random batches[1] of subjects are put through different conditions; in the second, a single batch is put through the different conditions successively; in the third, varied batches of subjects are put through the same conditions, as in correlational studies. The same three designs can be carried over into experiments on small groups instead of individuals.

(*a*) *Conditions varied, group constant: matched or random batches*

It is normal to use a control batch even when the experimental condition is unusual and it might be thought to be unnecessary. It ensures that the observed change of behaviour is really due to the experimental variable and not to the experimental situation, or to changes in time, or to other unnoticed factors. This design may instead be regarded as experimental variation of the situation between two batches of groups. With both matched and random batches a calculation of the statistical significance of the results must be made. Matched batches have no superior validity here, but as Thouless points out ((62), pp. 20–5) they may be more sensitive; if the statistical method for successive conditions of the same group is used there is a smaller variance, so that smaller differences are significant.

[1] The term 'batch' is used to avoid using 'group' in any sense other than 'social group'.

It is difficult in the present state of research to know for what dimensions of groups to match. This is because performance on the test used for matching should correlate with the performance chosen as dependent variable, and such correlations are as yet largely unknown. In general, temporary groups can only be matched for characteristics of the individual members, although Deutsch (16) matched the group as a whole by observing behaviour at the first meeting. It is hard to say how far behaviour at a first meeting is reliable as a guide to behaviour at later meetings. It is easier to match primary groups. Lippitt (38), for instance, matched for various individual characteristics and sociometric structure. Sociometry, though commonly used in these experiments, is rather inadequate since it only shows relations between people two at a time and since an arrow can convey very little of the true complex and two-way pattern of interaction between two people. The first difficulty can be slightly alleviated by Festinger's method (18) of matrix analysis for subgroups, and the second by using sociometric choices on different criteria—'best friend', 'to work with', etc.—and finding perceptions of status—'who is best at athletics?' etc.—as carried out by Polansky, Lippitt & Redl (47). The dimension of *group cohesiveness* (proportion of sociometric choices made within the group) has been studied as a variable and can be used as a matching criterion, as by Thibaut (60). There is need for further study of such group dimensions. Cattell (11) has developed concepts for describing group behaviour by factor analysis, but analytic concepts like group cohesiveness are more popular.

The difficulty of matching can be avoided by choosing a larger number of random groups (30–100) from a given population. This method has the additional advantage of greater generality and has become standard in recent work. However, as shown above, this method is less sensitive, and it is difficult to obtain such a large number of subjects.

(*b*) *Conditions varied, group constant: successive conditions*

While this design shares with matched groups the advantage of sensitivity, its basic drawback is that the first condition may transfer to the second. Often in these experiments it is not possible to correct this by counterbalancing since, as in Sherif's experiment (54), the sequence is important. There are three ways in which the first condition may affect the second. There may be some deception which cannot be repeated, there may be learning, for instance primary-group formation, and the subjects may object to their behaviour being changed experimentally and resist the effects of the independent variable. All three are acute in the case of small-group experiments. The third is perhaps the most serious and its effect is to reduce the influence of the experimental variable. An example is Berenda's study of the effect of the group on children's judgements (7): each child first judged the length of lines individually before judging in the second condition with a group of misjudging confederates of the experimenter; the memory of their previous judgements made them more resistant to social pressure.

Lippitt (38), however, used successive conditions as well as matched groups. Not only was this apparently successful but there were interesting transfer effects at the change-overs. The boys were unaware that an experiment was in progress, and it may perhaps be suggested that this design can only be used when this is the case, or when the attention of the subjects is directed away from the variables of the experiment. A good example of the latter is found in Festinger's study (17) of the effect of group-belongingness in a voting situation. The whole set-up was frankly artificial and experimental, but the purpose of the study was successfully concealed: after the subjects had voted by code numbers for

leaders the experimenter pretended to have made a mistake and after much hesitation agreed to disclose names and religions for use instead of code numbers. Votes were recorded ostensibly in secret but were in fact identifiable by the positions of the staples on the pads used.

In both of the first two designs different situations must be created. There are many ways of doing this, as will be seen from the summary in § (iii) below. A valuable addition to these procedures is investigation into how far the situations really were different for the subjects. This can be done, for instance, by rating the behaviour of the experimenter in cases where his behaviour is the differentiating factor as in the studies of Lippitt (38) and Thompson (61), or by interviewing the subjects afterwards as Festinger did (17). These are additional sources of information, as will be shown in another paper. The subjects' changed perception of the situation may not correspond with that intended by the experimenter, as Newcomb (45) points out.

(*c*) *Conditions constant, group varied*

This design is similar to the first in that two batches of groups are used which are matched or randomized in all respects save the one that is varied. French (23), for example, compared the effect of frustration and danger on primary and temporary groups, thus studying one of the most important dimensions of the group. Unfortunately, he failed to match for psychological sophistication—the temporary groups were composed of psychology students who were not taken in by the experimental arrangements. This may explain why the temporary groups showed less fear and aggression than the primary groups. Many other examples of variation of the group are to be found in the summary in § (iii).

(ii) *Generalization from the results*

The question of the validity of the conclusions must be separated from that of the validity of the experimental results. Thus Lippitt's experiment (38) has been illegitimately generalized by Lewin (35), while Wright (66) drew conclusions about the morale of the United States at war from an experiment on the effects of frustrating pairs of pre-school children. Criticisms made of small-group investigations are very often found to be criticisms of such invalid generalizations. Further, in any attempt to survey the results of research in a certain field or to base theory on such results, attention must be paid to the generality of each investigation. In addition to the five problems below there is a sixth, generalization to groups of greater size, which has already been discussed in the Introduction.

(*a*) *The 'artificiality' of the laboratory situation*

While in a sense no situation is any more 'real' than any other and laboratory results are quite genuine, people may be affected by the laboratory situation and behave unusually, in which case the results cannot be generalized to explain real-life situations outside. This applies more in the case of social experiments because social behaviour is more 'projective', i.e. less controlled by the task, and there is some resistance to having one's social behaviour changed experimentally. There are several ways of minimizing this effect. In the first place children can be used; Lippitt (38) introduced his observers to the groups of children with: 'There will probably be people over there in the other part of our clubroom quite often who are interested in seeing how a good club runs. They'll have

plenty to do and so will we; they won't bother us and we won't bother them' (p. 67). Occasionally with adult groups it is possible to perform the experiment without their knowledge, as with the group decision studies (24, 34). Small groups in the field are extremely sensitive to investigators and a fuller discussion is needed of the role the social scientist should take. Experiments in the laboratory with adult groups must necessarily have an unreal flavour about them, but under certain conditions the effect of this may be mitigated by concealment of the purpose of the experiment and by such means as one-way screens, as for example in French's experiment (23).

(*b*) *Generalization from temporary to primary groups*

Most experiments have been performed on temporary groups, the alternative being to find suitable primary groups already in existence as in the case of Coch & French (13), or to create primary groups over a period of time as Lippitt did (38). Until more is known about the process of formation of primary groups, and more experiments like French's have been done comparing them to temporary groups, it is impossible to say how far one can generalize from one to the other. Studies of temporary groups can either be regarded as studies of social behaviour in the culture or as studies of incipient primary groups; the latter attitude is taken in § (iii), since in the course of most temporary-group experiments there is some degree of primary-group formation.

(*c*) *Misinterpretation owing to the limitations of the particular design*

While the purpose of an experiment is to isolate causal sequences, the design may encourage unusual forms of behaviour which will attract undue attention. This criticism can be made of Sherif's studies of the formation of social norms in totally unstructured situations, i.e. with no objective or reality component (54). Such situations are rare, but Schonbar has confirmed Sherif's findings in a structured situation (53). In general psychology there are familiar instances where experiments have been designed from the point of view of a particular theory and have emphasized appropriate processes. These difficulties can be avoided by restricting the generality of the results to the precise conditions of the investigation until they have been found to hold under varied conditions ((64), pp. 378–9). Obtaining greater generality for the results and taking care that a freak process has not been discovered are part of the same process.

(*d*) *The effect of the personality of the experimenter*

The experimenter may have a way of eliciting unique social behaviour. French confesses ((24), p. 87) that his group decisions were not so effective as those of Bavelas, and Maier found great differences between group decisions with trained and untrained leaders (39). This effect can be controlled by alternating different experimenters or obtaining ratings of their behaviour, as mentioned above. It is worth while to treat the experimenter as a variable when the style of his behaviour is an important factor.

(*e*) *Representativeness of the sample*

One reason for varying the group is to increase the generality of the conclusions. It is well to do this at the same time as varying the conditions, as Schachter did (52); this has the additional advantages of economizing in experimentation and disclosing interaction between the variables, as pointed out by Fisher ((22), pp. 90 f.). Greater generality is

given by random than by matched batches even if less specific information is obtained—for it is not known for which precise sub-batches the causal relation does and does not hold.

Even if the group is varied, the individuals concerned may be drawn from a narrow section of society, e.g. second-year students of a faculty in some university. At best the results will be representative of groups in that subculture. It is interesting to consider generalization outside such a subculture. Social behaviour depends on the culture more than does the behaviour usually studied in experimental psychology laboratories, and if a psychological explanation is given, this in turn depends on the cultural conditions (63). These can be considered in two parts, the influence of child training and the influence of present social norms.

Child training leads to general personality traits such as competitiveness and co-operativeness, which may be general for the culture (42). It is impossible to reproduce such childhood experiences in the laboratory, and experiments would have to be repeated in other cultures to obtain more general results. Strodtbeck (57) treated the culture as the major variable in his study of husband-wife interaction, but it has never been treated as a subsidiary variable for exploring the generality of some other experimental result. It is, however, possible to reproduce specific motivating conditions, as by Deutsch in his study of co-operation and competition (16). This can be held to reproduce variations along one dimension in which cultures differ (42).

The influence of present social norms has been studied by Sherif (54) and Merei (43) who allowed them to be set up experimentally. It could also be done by conducting field experiments against different institutional backgrounds, though this has not so far been done. With this kind of investigation a distinction could be made between universal laws of social behaviour and those dependent on certain cultural conditions.

(iii) *A brief survey of experimental work on small groups*

These experiments may be conveniently classified according to the independent variables studied in them. The latter may be in the environment or in the members, and further subdivisions are suggested below. Our focus of interest will be the primary group at equilibrium with its environment, in the sense described by Henderson (28) and Homans (29). Temporary groups will be regarded as incipient primary groups moving towards such a stable system. Again there are three kinds of study of primary groups, those of the development, persistence or change of the social system of the group.

(*a*) *Independent variables in the environment*

A persistent background factor to the activity of primary groups is the culture, especially that area of it consisting of beliefs and customs about small groups, as Bion (8) has suggested. The only experiment in this field is by Strodtbeck (57) who showed that husband-wife interaction varied with the relative dominance of the sexes in different cultures.

The ecology, the extent to which the environment satisfies or frustrates needs, has been studied mainly by non-experimental methods. French (23), however, in an experiment which has been criticized above, showed that primary groups became more frustrated by insoluble problems and showed more fear and aggression in a danger situation than

temporary groups. Wright (66) found that while pairs of strong friends showed more aggression to outsiders when frustrated, all pairs became more cohesive.

The positions of the members in the wider social structure may also be regarded as an environmental variable. Strodtbeck's study, mentioned above, illustrates this, though more interesting is Festinger's comparison (17) of the effects of revealing religious affiliations on Catholics and Jews in a voting situation. Swanson (59) found some evidence that increasing the range of social status in a group reduces friendliness and ease in making decisions.

The position of the group itself in the social structure should also be considered. Such a position may be the result of formal organization or of mutual adjustment in the community. Thibaut (60) and Kelley (31) created miniature hierarchies and found, for instance, that subordinated groups showed hostility and communicated irrelevant material upwards as a substitute for mobility.

Lippitt's classic experiment (37, 38) on autocratic and democratic atmospheres has been followed by several related studies: Thompson (61) found greater social adjustment as a result of democratic teaching methods, while Preston & Heintz (48) and Bovard (10) found that democratic leadership facilitated norm-formation. There is also an interesting study by Merei (43) who found that in groups of young children leaders had to lead within the norms.

There is a group of experiments under the inspiration of Bavelas (5, 6), studying the effects of different patterns of communication in small groups. Leavitt (33) and Heise & Miller (27) found that patterns varied in efficiency for group problem-solving and that in some cases the leadership was dictated by the pattern.

Lastly, the mode of formation of the group constitutes an important variable—whether the group is recruited by birth, organized by external authority or formed spontaneously from informal contacts. Most experiments of course have dealt with groups of the second variety.

(*b*) *Independent and quasi-independent variables in the members*

If the development of a small group is being studied, aspects of the members can truly be regarded as independent variables. At later stages, and when the persistence and change of the group are in question, while such aspects may still be experimentally manipulated they are only what may be called 'quasi-independent variables', being themselves dependent on other parts of the social system.

For instance, while the attitudes, judgements and non-social behaviour of members may initially be an important determinant of mutual perceptions and interactions, they are quickly modified by the formation of social norms, or 'levelling'. Sherif's demonstration (54) of this for perceptual judgements in the unstructured autokinetic effect has been confirmed by Schonbar (53), Berenda (7) and Asch (2) in structured situations, and had been shown earlier for task performance by Lorentz (cf. (15), pp. 1111–13). Of far greater interest is the series of studies by Festinger and his associates Schachter and Back (17, 3, 21, 51) of the process of social control whereby norms are set up and enforced. Deviating members in discussion groups received the bulk of communication and either conformed or were rejected, particularly in cohesive groups. Schachter, Ellertson & Gregory (52) showed that cohesiveness does not in itself lead to increased production but merely increases the control of the group over its members—which may lead to increase or decrease. With the

exception of a rumour experiment ((20), chap. 7) which has been criticized in detail elsewhere (1), these investigations were conducted with great skill and were co-ordinated with respect to theory.

Related to social control is the study of group decision as a technique for changing the behaviour or attitudes of a group by altering the point at which social control operates. Lewin's demonstration (34) of its effectiveness as a way of changing the food habits of housewives is supported by studies by Coch & French (13) and Bavelas (cf. (24)) who used it to increase industrial output. Maier (39), as mentioned above, found that its effectiveness depended very much on the skill of the leader.

The interpersonal attitudes of members and their motivation in the group do not exist before the social system of the group has formed and so can only be quasi-independent variables. Mintz (44) found that panics ensued when certain situations were perceived as competitive, rather than as a result of emotional excitement; Deutsch (16) noted that co-operative groups were more friendly and productive than competitive ones, while Stendler, Damrin & Haines (56) found fewer ego-demands and less boasting in co-operative groups. Harvey & Sherif (26) discovered that the more ego-involved two people were, the more alike were their mutual estimates of future performance. Lastly, Horowitz, Lyons & Perlmutter (30) found that attitudes towards events in groups depended on attitudes to the agents of the events. This area of small-group research is the least studied and may turn out to be the most fruitful.

The final independent variable is that of the personalities of the members, especially those aspects concerned with social interaction. There are no detailed studies of the ways in which a number of people discover an harmonious and stable pattern of interaction, but it may be supposed that there is 'hypothesis behaviour' as various patterns are tried out until a satisfactory one is found. In this case it is unlikely that group behaviour will be predictable from knowledge of the individual members save in a very complex way. However, Swanson (59) and Brown (cf. (59)) successfully predicted behaviour of individuals in groups from scores on personality tests, and Rosenthal & Cofer (50) found that the presence of an indifferent member reduced the belief of the group in the attainability of its goals. There are other indications of the importance of individual differences, such as the demonstrations by Bovard (9) and Asch (2) of differences in acceptance of group norms and the recorded behaviour of operative 2 in the Hawthorne experiment (49, 65). Crutchfield (14) has recently suggested the use of a group method for personality testing which is more standardized than, for instance, the W.O.S.B. Small-group research is just emerging from a phase when too little attention was paid to individual differences, perhaps as a result of thinking in terms of social norms and in terms of Lewin's concept of the 'social field' (36), which assumes complete homogeneity of members.

III. Conclusions

The principal conclusion is that experimental techniques for the study of small social groups are now firmly established despite the difficulties. Further, it is clear from the brief survey given above, § (iii), that large areas of research are relatively untouched while others contain much repetition and proof of what is obvious. Again many experiments while in themselves brilliant and original are quite unrelated to any others, and it is only in the single field of social control that there has been any attempt at integration of research with respect to theory. Clearly there is need for a more comprehensive

deductive theory of the social systems of primary groups; it may be suggested that a fruitful form of such a theory would be in terms of individual processes, since more is known about these than about small groups, and since this would provide a further degree of explanation. Lastly, it may be pointed out that since most laboratory experiments suffer from the double artificiality of the laboratory atmosphere and the use of temporary groups there is need for carefully controlled field studies.

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A CONFIRMATION OF THE ERGIC AND SELF-SENTIMENT PATTERNS AMONG DYNAMIC TRAITS (ATTITUDE VARIABLES) BY *R*-TECHNIQUE

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I. *Historical roots of research on ergs* (pp. 280–281). II. *The action definition and ergic measurement of attitudes* (pp. 281–282). III. *The experiment and statistical analyses* (pp. 282–286). IV. *Discussion of ergic patterns presented by the factors* (pp. 286–290). V. *Summary and view of further research* (pp. 290–293). *References* (pp. 293–294).

I. Historical roots of research on ergs

The notion of instinct, as ultimately refined in McDougall's concept of propensity, has suffered curious vicissitudes. To understand these realistically one must glance at more and less than rational considerations, affecting psychology as a sociological phenomenon. For example, it is noticeable that these swings of fashion, like most, were greater in the United States than in Britain. Further, the remarkable American vote against propensities, in the debate between McDougall & Watson (21) may be connected with Boring's observation (2) that psychology is treated with more professional rigour in the United States and more amateur individualism in Britain. For the rigorous pursuit of a reflexological cult would necessarily eliminate propensities, whereas only the comprehensive common sense and individuality of the 'amateur' could save the propensity concept at a time when it rested on holistic thinking and not in the least on any precise laboratory demonstration. The last but by no means the least of the forces which accounted for this valuable concept being 'buried alive' (10) resided in the sociologists who, in America, were driven by socio-political immediacies (connected, as M. Culpin (7) has observed, with the absorbtion into the United States of a large influx of diverse races) into denying innate mental characteristics wherever possible (1,18).

Whatever may be the causes, the fact remains that historically the notion of propensity fell out of psychology, in varying degrees in various countries, and then returned, in the ghostlike form of 'tensions', 'drives', 'needs', etc. So central is the notion of propensity, however, to any systematic psychodynamics, that the recent growth of experimental work in psychodynamics (11,16,20) and motivation phenomena generally (15,23,26) is now forcing us to attempt once more a clarification of concepts regarding the behaviour which used to be included in the notion of instinct.

No matter how much one may disagree, from the standpoint of a common-sense integration of available evidence, with the refusal to talk about propensities, one must admit that the objectors were right in demanding a more scientifically acceptable rigour in the *methods* of demonstration.

Unfortunately, in McDougall's time, and for some people even now, scientific method meant only 'controlled experiment'. Actually, science is a two-handed affair, of controlled experiment when that is feasible, and statistical analysis of the uncontrolled 'observations *in situ*' when it is not. And the advantages are not all with the brass instrumentality of the first, classical, physical-science, univariate method. Indeed, the most important of the influences in clinical and social science data are inferable only from a multivariate

design and by attending to the patterns discoverable among many variables by some sort of 'holistic' statistical approach. The device which has made the holistic approach scientifically practicable and has now brought what was merely 'common-sense' appraisal of a mass of influences into a new realm of precision and demonstrability is *factor analysis*. The classical experimental method works with pairs of variables, the dependent and independent, but factor analysis may take a hundred variables at once and demonstrate the structure of the relations among them.

To anyone familiar with the principles of factor analysis and their success in the realm of abilities it was evident by 1932, when the funeral rites were being performed over propensities, that, at least in theory, this method was ideal for dynamic investigations, and the present writer so urged it upon McDougall, as a possible scientific defence of propensities. Since then the structuring of personality generally by this method has offered additional hope of the practicability of dynamic analysis. Accordingly, in 1946, the Laboratory of Personality Assessment undertook a first application to purely dynamic data. Two hundred young men were measured on each of fifty interest-attitudes, the latter being chosen to represent the hypothesized drives of such writers as McDougall and Murray, as well as certain commonly supposed sentiment structures. The results (10) agreed remarkably well with the hypotheses as to drives, but not as to sentiments. However, since factor analysis is open to charges of some subjective 'flexibility' in interpretations it seemed desirable to repeat the inquiry in an independent experiment, which is now reported in this article.

II. The action definition and ergic measurement of attitudes

The experiment reported here has three noteworthy points of design: (1) employment of the action definition of attitude, which distinguishes the method from that of most current attitude studies using such concepts as those of Guttman, Stouffer (27), Likert (19) or Thurstone (28); (2) new *objective*, non-opinionaire, methods of measuring attitudes; and (3) the development of the concept of erg (3,6,10) instead of propensity, and the resulting 'ergic system of attitude measurement' (6), treating attitudes as vectors.

The dynamic manifestations which offer the most suitable variables or elements among which to seek the larger structures are attitudes. For reasons given in detail elsewhere (6,8) we have rejected the definition of an attitude (as in Guttman, Likert, Thurstone, and others) as a feeling 'for or against an object'. This is derived from the preoccupation of applied psychology with polling; it shows that lack of reference to personality structure hypotheses which would be expected from considering paper rather than people; and it creates artificial problems of measurement, e.g. through failing to distinguish strength and direction. Instead, we take the paradigm: 'In these circumstances I want so much to do (or to have done) this with that.' This indicates (1) a *stimulus* situation, (2) an *organism*, (3) a dynamic trait of given *strength*, and (4) *a course of action* in relation to an *object*, which fixes the *direction* of the dynamic trait. The literal course of action, with its object (which course, incidentally, distinguishes an operationally observable, single attitude from an inferred dynamic trait with no single action or object) *fixes* the attitude direction but does not, so far, define it in any wider context. The wider definition, if our experimental finding of independent ergic factors is substantiated, can be in terms of *direction with respect to a set of ergic co-ordinates*, as illustrated elsewhere (6,10,11,15). Thus the attitude is represented as a vector, the length of which expresses its strength and the direction its

ergic quality, i.e. the emotional goals sought. Attitudes constitute the initial structures in the subsidiated traits of the dynamic lattice (10).

It has been admitted by users of current opinion-attitude methods that their mode of scoring loses much of the richness of the ergic method. 'For' and 'against' are granted to be mere black and white substitutes for a coloured photograph, since most of our attitudes, e.g. to a countryside or a glass of beer, are far more complex than 'for' and 'against', e.g. we are 'curious about', 'sociable towards' 'proud of', etc. This admission, however, misses the more subtle point that one can be for or against *a course of action*, but not, in the same sense, for or against an *object*. To be against an object can surely only be interpreted in terms of wanting to destroy it. Anger, fear or disgust are distinct needs to get rid of an object in different ways, and none of them is a mere opposite of the dozen or more goals for which one would want an object to continue to exist. An interest in the continued existence or the destruction of an object we have called a 'sentiment', since in general it derives from an organization of *many* attitudes as 'courses of action about the object'. Thus we need to distinguish being 'For or against a course of action, connected with an object' (attitude) from being 'interested in preserving or destroying an object about which *many* attitudes simultaneously exist' (sentiment). The latter, the extent of interest in preserving the object, is presumably the sum of interests in the several attitudes affecting the object, and is probably most easily measured by first measuring the latter, as vectors.

This is no place to pursue further the intricate and debated issues of the dynamic lattice (6), but at least we must distinguish clearly the use of 'attitude' on which this study is based. To summarize: in the ergic system of attitude measurement an attitude is a vector, the length of which is the strength of an interest and the ergic direction of which is the conative quality of the interest. The action definition of attitude requires that *a course of action in relation to an object* be fixed before measurement begins and that the 'strength' then concerns only that particular course of action. It will be realized, incidentally, that this definition brings attitude measurement within the standard frame of reference of behaviouristic (as distinct from reflexological) psychology in the laboratory. It also relates it to clinical (but not the sociological valence) study of motivation. For an attitude becomes a response to a stimulus—$R = f(OS)$—dependent for its strength on the stimulus situation (S) and the state of the organism (O).

III. The experiment and statistical analyses

Our experimental design calls for the measurement, by objective means, of the strength of a sufficient collection of attitudes defined in the above way, on a sufficient population of persons, with an ensuing intercorrelation and factorization of the attitudes. Recognition of the nature of the factors will depend on the accumulation in one factor of attitudes having courses of action which are similar, which, further, are known from common introspection to draw on a specific emotion, and which are known from the recognized subsidiations of our culture pattern to contribute to a common goal. The stimulus situation in all cases is that of the individual's fixed life situation, considered reasonably constant during the experiment.

It will be recognized that the use of an R-technique factorization, i.e. correlation of diverse variables, as to strengths, in a population of persons, presupposes that the strengths of any erg will vary from one person to another, *as a whole*, either through differing intensities of provocation by the life situation in relation to opportunities for

satisfaction or through differing constitutional and physiological conditions. An alternative design would be to factor analyse a single individual, using *P*-technique (5), depending therein upon the variation in ergic strength from day-to-day with stimulus and physiological conditions, and this might even be expected to give clearer variation. The latter (14) has actually been recently carried out with comparable variables and gives very similar results, as can be seen by comparing the factor matrix with that of this article.

Success in factor analysis in getting the maximum structuring and information depends on a right choice of variables. Indeed, it is sometimes overlooked that factor analysis is a means of *testing hypotheses*, stated in terms of the variables (13). The pilot study to the present (11), as mentioned above, took a wide range of interests, in fifty attitudes, specifically chosen to mark each of the propensities defined clinically by McDougall (22), Freud (17), Murray (24) and others, as well as the chief sentiment structures suggested by the sociologists' surveys of institutions. That study failed to find any sentiment except the self-sentiment, and since our interest here is in any case primarily in the structure of ergs we took from the previous study *two* (or in special cases three) attitude markers for each of the *ergic* patterns there found, plus two for the self-sentiment. That is to say, we took as variables here two or three of the highest loaded variables for each of the nine factors there found, keeping them identical in definition in order that an exact comparison might be made between the two factorizations.

In this final designing of the experiment it was necessary to take careful stock again of our main objectives. The situation in which we found ourselves was that a pioneer study had revealed patterns in dynamic traits strongly suggestive of the hypothesized ergs. It was extremely important that an independent check be made at the first opportunity to ensure that this result was not fortuitous. Such a check would have greatest validity: (1) if we took a somewhat different population, for by definition ergs are universal in character; (2) if we took the majority of variables *identically* defined, but also added one or two new attitudes perhaps better defining the essence of one or two ergs not previously well represented (for our objective was not only to check the previous patterns but to substantiate that they are specific ergs); (3) if, while retaining objective tests of sufficient validity as motivation measures we changed the modes of measurement somewhat, to make sure the pattern was not dependent on any particular artifact of the strength-measuring device; (4) if we made the factorization independent and, above all, *sought simple structure by blind rotation* without guidance from meanings found in the previous study.

By way of population variation we took male undergraduates in place of the married veterans of the previous study, and 73 of them completed the three hours of experimentation. The list of 30 attitude variables finally chosen is set out in Table 2, where they are arranged to show their loadings on the erg, in the initial study which they were chosen to represent, as well as their subsequent loadings on the factors of this study. As to the objective measurement devices, we took two identical with the previous study, namely, 'preference' and 'information', and one different, namely, 'fluency' (chosen to be simultaneously applicable in the parallel *P*-technique research (14), completing the triangle of related researches).

The tests were administered in the order in which they are described below. In the preference test each attitude was paired in turn with every other and the individual indicated which course of action and satisfaction he preferred. The pairs were randomly

ordered with the provision that no attitude would appear in two consecutive pairs. The subject was allowed up to one hour to complete the 435 choices on the pairs of attitudes. The score for any attitude was the number of times it was preferred.

The information test presented questions concerning facts assumed likely to become known by anyone who might be strongly and habitually interested in the given course of action. It consisted of 125 choice items randomly ordered, with each of the 30 attitudes represented by 4 items. The individual was given 15 sec. to read each item and to indicate the best choice as it was flashed on a screen. The score for an attitude was the number of items (out of four) correctly answered. Each individual's attitude scores were ipsatively (4) scaled, i.e. divided by his total, general information score, on the assumption that the total life interest for each individual is not the same.

In the fluency test the subject was given the sentence defining the attitude and asked to write as many justifications for the attitude (including further satisfactions to which it would lead) as possible in each of two 1 min. periods. The total number of ideas per attitude was again ipsatively scored to cancel individual differences in total fluency.

It will be observed that contrary to most attitude measurement practices we based the estimate of the strength of the individual's attitudes on three distinct types of contributory test, instead of upon one only, the usual verbal test. Our theory is that the strength of the interest or motive is best measured by whatever is common to these different manifestations. Unfortunately, we encountered a difficulty here not revealed in the previous research, namely, that for *some* attitudes the correlations for one of the three different tests would be negative in relation to the others, yielding a mean r for *all* 30 attitudes still positive but not significant. There is urgent need for a great deal more research on the intricacies of the objective motivation measurement processes themselves, which our pioneer studies (8,9) have treated rather roughly, our main interest lying in the *structuring* by factor analysis. However, where the correlations were unsatisfactory (insignificant or negative) we based the measurement on the Preference test alone, which is the most valid according to previous research. This occurred for attitudes 5, 8, 10, 15, 16, 21, 26 and 29. Attitudes 5, 8, 10, 15, 21, 25, 28 and 30 have all been omitted from the average in Table 1*a*. Fortunately, except for 29, these play no important further role in any of our factor definitions.

The mean r's for the three methods on the remaining attitudes are shown in Table 1*a*. As a partial check on how far the pooling or not pooling of uncorrelated measures of the same attitude affect the resultant factor-loading patterns, the information score for attitude 15 and the fluency score for attitude 26 were inserted separately as variables 31 and 32. It appears from the latter that despite definitely poor intercorrelation the different measures *can* factor very similarly. Indeed, in adding measures with no large intercorrelation in the given sample (though adequate in our previous larger sample) we are only doing what is commonly done in applied psychology where tests without intercorrelation are considered to measure different aspects of the same thing, e.g. different measures of morale. For even to depend on so small a communality we are doing better than to depend, as is common, on a single measure—verbal opinion statements—with obvious large specificity. Moreover, the factor analytic method theoretically yields the same factor structure, though with much reduced loadings, for correlations attenuated by low reliabilities as for those of high reliability, and in practice has been found to do so (13). Reliability in this sense corresponds to validity in ours, for by validity here we

refer to agreement between two different test devices aiming to measure the same thing, namely, the strength of interest in the course of action defined by the attitude. The actual reliabilities of the three devices themselves are set out for every attitude in Table 1*b*. Two of the methods are invariably good by usual standards, but the third is poor. These figures suggest the contingent conclusion that the fluency method fails through some systematic invalidity (the tendency to run negative for certain attitudes), but that the

Table 1. *Reliabilities and validities of the objective measurement devices*

(*a*) Intercorrelations—validities of devices—mean of 30 attitudes

	(1)	(2)	(3)
(1) Preference	—	—	—
(2) Information	0·016	—	—
(3) Fluency	0·092	0·011	—
Average	0·054	0·014	0·052

(*b*) Reliabilities of devices, all attitudes singly[1]

Attitude number	(1) Preference	(2) Information	(3) Fluency
1	0·896	0·032	0·735
2	0·641	−0·114	0·766
3	0·819	0·971	0·744
4	0·930	−0·051	0·766
5	0·799	0·133	0·790
6	0·832	0·268	0·701
7	0·836	0·160	0·762
8	0·890	−0·145	0·801
9	0·758	0·270	0·749
10	0·696	−0·198	0·749
11	0·945	−0·190	0·790
12	0·738	−0·088	0·749
13	0·860	0·124	0·686
14	0·937	0·037	0·726
15	0·860	−0·043	0·766
16	0·856	0·123	0·793
17	0·805	−0·407	0·774
18	0·820	0·019	0·753
19	0·847	−0·108	0·721
20	0·812	0·415	0·740
21	0·831	0·408	0·691
22	0·587	−0·051	0·731
23	0·738	0·347	0·641
24	0·872	0·112	0·770
25	0·934	0·223	0·744
26	0·871	0·068	0·753
27	0·740	0·148	0·762
28	0·908	0·068	0·821
29	0·837	0·081	0·681
30	0·942	0·424	0·735

[1] Reliabilities for the preference and fluency measures were calculated by a split-half product moment with Spearman-Brown correction for double the length. The information reliability was computed by Kuder-Richardson formula,

$$r = \frac{n}{n-1}\left(1 - \frac{\Sigma pq}{s^2}\right),$$

where n = the number of items, p = the proportion of the group passing each item, q = the proportion of the group *not* passing each item.
s^2 = the variance of the scores.

information test, previously found valid (8), simply fails to give validity through being too brief (only four items here). Until better methods are known further researches must simply give longer tests.

When the strength of each attitude for each person had been measured, by combining (see description above regarding which modes are combined) his three sub-test scores, with a weight of three for the Preference and one for each of the Fluency and Information

Table 2. *Attitude variables as loaded in previous and in present rotated reference vector matrix*

Variable number	Definition	Previous identification	Previous loading in identified erg	Factors								
				1 Gregariousness	2 Self assertion	3 Curiosity	4 Mating	5 Escape	6 Super-ego (?)	7 Appeal	8 Parental protectiveness	9 Self-sentiment
1	I want to continue in America the most congenial country for me in people and ideals	Parental protective	0·53	0·07	0·01	−0·18	−0·10	0·29	0·07	−0·31	−0·07	−0·01
2	I want to be able to insure for my children the best education that they could possibly have	Parental protective	0·48	0·04	−0·16	0·04	0·10	0·00	−0·40	−0·02	0·40	−0·04
3	I want my parents never to be lacking the necessities of comfortable living	Parental protective	0·44	−0·10	−0·20	0·08	−0·07	−0·21	0·07	0·00	0·51	0·15
4	I want to help the distressed wherever they are	Parental protective	—	−0·51	−0·51	−0·02	0·24	−0·06	0·07	0·01	−0·09	−0·01
5	I like to go to a good movie or play every week or so	Mating	0·54	0·32	0·09	0·35	−0·10	−0·09	0·15	−0·20	0·50	0·03
6	I want to make love to a woman I find beautiful	Mating	0·41	0·02	0·09	−0·21	0·42	−0·18	−0·32	−0·07	0·45	−0·02
7	I like sex appeal in a woman	Mating	—	0·06	−0·27	−0·01	0·49	−0·15	−0·42	−0·17	0·09	−0·04
8	I like to take an active part in athletics and to get exercise	Gregarious	0·69	0·49	−0·07	0·04	−0·08	−0·25	−0·00	−0·04	−0·04	−0·09
9	I want to attend football games and follow the fate of teams	Gregarious	0·42	0·48	−0·15	−0·02	0·01	−0·14	0·02	−0·10	−0·10	−0·08
10	I would rather spend free time with people than by myself	Gregarious	—	0·30	−0·04	0·18	0·06	0·04	0·28	−0·28	0·05	−0·04
11	I want to feel that I am in touch with God, or some principle in the universe that gives meaning and help in my struggles	Appeal-dependence	0·67	−0·10	−0·23	−0·28	−0·29	0·09	−0·10	0·53	0·02	−0·03
12	I want to have my parents' advice and to heed their wishes in planning my affairs	Appeal-dependence	0·41	0·01	−0·25	0·01	−0·13	0·04	0·04	0·47	−0·44	−0·09
13	I like to know a sure place to turn for help when difficulties become too overwhelming	Appeal-dependence	—	−0·13	−0·03	0·09	0·00	−0·08	−0·09	0·03	−0·24	0·32

14	I want to see organized religion maintain or increase its influence	Appeal-dependence	0·62	−0·07	−0·03	−0·16	−0·17	−0·09	0·32	0·26	−0·37	0·08
15	I want to listen to music	Curiosity	0·52	0·18	−0·03	0·52	−0·08	−0·01	0·25	0·08	0·27	0·07
16	I want to know more science	Curiosity	0·50	−0·31	0·09	0·33	0·01	−0·08	0·09	−0·24	0·23	0·01
17	I would like to satisfy my curiosity about everything around	Curiosity	—	−0·21	−0·07	0·50	−0·21	−0·09	0·03	0·01	0·22	−0·29
18	I want America to get more protection against the terror of the atom bomb	Escape and fear	0·52	−0·15	−0·05	0·04	−0·05	0·49	−0·07	0·28	−0·08	−0·17
19	I want to see any formidable militaristic power that actively threatens the U.S.A. attacked and destroyed	Escape and fear	0·49	0·00	0·02	0·02	−0·27	0·45	0·26	0·20	−0·15	−0·45
20	I want to see the danger of death by accidents and disease reduced	Escape and fear	—	−0·27	−0·24	−0·03	0·27	0·35	0·01	−0·04	0·10	−0·03
21	I want to spend somewhat more on drinking and smoking than I am now able to do	Regressive, narcissistic	0·56	−0·01	−0·04	−0·02	0·36	0·04	−0·04	−0·22	0·52	−0·10
22	I would like to spend more on really fine foods, candy and delicacies	Regressive, narcissistic	0·47	0·09	0·06	0·03	0·37	−0·31	−0·09	−0·35	0·08	0·17
23	I like to enjoy the luxury of my bath at leisure	Regressive, narcissistic	—	0·27	0·10	0·25	0·07	0·03	−0·09	−0·05	0·08	0·06
24	I want more time to enjoy my own company, my own thoughts and reveries	Regressive, narcissistic	0·34	−0·21	−0·15	−0·01	0·01	−0·28	−0·10	−0·23	0·37	0·02
25	I want never to do anything that would damage my sense of self respect	Self sentiment	0·44	0·33	−0·04	−0·09	−0·04	−0·03	−0·09	−0·29	0·30	0·04
26	I want to get a good reputation among those in my profession who can help my advance	Self sentiment	0·32	0·10	0·72	−0·05	0·03	−0·12	0·29	−0·08	0·08	0·24
27	I like to keep good control over my mental processes, e.g. memory	Self sentiment	—	−0·49	−0·02	−0·22	0·00	−0·10	−0·09	−0·09	0·05	0·60
28	I want to increase my salary	Self assertion and display	0·31	−0·41	0·47	−0·07	−0·26	−0·08	−0·07	−0·02	−0·02	0·19
29	I want to become proficient—if possible to excel my colleagues in my chosen profession	Self assertion and display	0·33	−0·07	0·59	0·07	−0·17	0·27	0·36	0·09	−0·17	0·29
30	I like to have command of people	Self assertion and display	—	−0·03	−0·03	0·00	0·05	0·21	0·05	−0·13	−0·31	0·19
	±0·10 hyperplane (number of variables falling in hyperplane)			15	19	21	18	17	20	16	15	19

tests, the attitudes were inter-correlated, yielding a range of values from −0·523 to +0·584. The correlation matrix was factored by the Complete (simple) Centroid method (13), yielding nine factors by Tucker's criterion of completion of extraction. These were blindly rotated for simple structure, requiring seventeen overall rotations, and were slightly adjusted in three more pre-informed rotations to converge better on structures simultaneously becoming evident in the corresponding *P*-technique study (14). The structure was quite definite enough, but with as few as 30 variables the exact angle of a hyperplane naturally cannot be defined as well as it was in the 50-variable previous study (11). The rotated reference vector matrix resulting is shown in Table 2 and the angles among the reference vectors in Table 3.

The correlations among the factors (reference vectors) were of average magnitude for such oblique solutions, as shown in Table 3, there being some resemblance to the previous study in a slight positive correlation of the ergs of Gregariousness and Self-Assertion and the ergs of Mating and Parental-Protectiveness (also found in *P*-technique (14,15)).

IV. Discussion of ergic patterns presented by the factors

An inspection of Table 2 leaves no doubt that the grouping of the attitudes is substantially the same as in the previous study, while most of the new hypothesis-testing variables added agree with the ergic interpretations previously adopted. Understandably, actual loadings are not the same, for differing reliabilities of tests and differing samples preclude this; but the attitudes which were expected to mark a given factor in general have significant loadings in that factor and no significant loadings anywhere else.

Judged in this holistic way, six of the factors meet the requirement of a good match with the previous study, while the remaining three have slight but noticeable similarity in loading pattern to those found earlier. The good matches are: (factor 1) the gregarious erg (compared on attitudes 8 and 9); (factor 5) the escape and fear erg (attitudes 18 and 19); (factor 2) the self-assertion erg (attitudes 28 and 29); (factor 3) the curiosity erg (attitudes 15 and 16); (factor 4) the mating erg (attitudes 6, 7 and 22); and (factor 7) the appeal-dependence erg (attitudes 11, 12 and 14).

This leaves three old factors, parental erg, narcissism and self-sentiment, unaccounted for, and three new factors 6, 8 and 9, unmatched. Some discussion is necessary on these, since the markers do not come out neatly in a single factor, as in the other instances (except mating), clearly confirming hypotheses. The parental-protective markers, attitudes 1–4, have their highest mean loading in factor 8 (only approached by factor 2), but the pattern is contaminated by high loading positively for the sex markers (5 and 6) and negatively for appeal (12–14). Since these two factors have better loadings elsewhere it is conceivable that by deliberate 'rotation for meaning' this variance could be shifted into the factors to which they belong, at the cost of rather strong positive obliquities of parental and sex factors and negative obliquities of parental and appeal. (The former has indeed been found elsewhere (14); the latter is perhaps unnecessary if we accept the psychological interpretation that love for others (agape) precludes concern about the self and directly negates appeal behaviour).

The self-sentiment variables (25–27) have their highest mean loading (+0·28) in factor 9, but again the pattern is contaminated, as shown below. However, the fact that the substitute variable for 26, namely, 32, is also significantly loaded here, leaves little doubt as to the identification.

In the above, a negative loading, i.e. *not* wanting, is written as inhibition, and if we accept the inhibitory role of the self sentiment, and that revenge and inquisitiveness are much inhibited, the two chief 'contaminators', 19 and 17, can be explained. The succouring of the self, no. 13, is also perhaps a necessary part of the self sentiment. This self-sentiment pattern, if the match is to be accepted, is less concerned with self respect and more with Spartan self control and ambition than that found for the somewhat older and married men (Table 2).

As to the hypothesized narcissistic erg, the highest mean loadings of its four (*R*- and *P*-technique) markers occur about equally in two factors, 7 and 8, and these are already matched. The highest for the original *R*-technique markers occur in 7 and 4, also matched. If we decline to put down this dispersion wholly to the chance unreliability of our measures,

Table 3. *Direction cosines among reference vectors*

		1	2	3	4	5	6	7	8	9
Gregarious	1	—	—	—	—	—	—	—	—	—
Self-assertive	2	0·48	—	—	—	—	—	—	—	—
Curiosity	3	0·30	0·31	—	—	—	—	—	—	—
Mating	4	−0·31	−0·43	−0·33	—	—	—	—	—	—
Escape	5	0·19	0·14	0·22	−0·07	—	—	—	—	—
Super-ego	6	0·23	0·25	0·32	−0·40	0·01	—	—	—	—
Appeal	7	−0·07	−0·17	0·28	0·16	0·14	−0·05	—	—	—
Parental	8	−0·06	0·09	−0·08	0·01	0·05	−0·13	−0·18	—	—
Self sentiment	9	−0·13	0·13	0·35	−0·07	−0·44	−0·24	−0·22	0·01	—

Table 4. *Highest variables in presumed self-sentiment pattern* (*Factor* 9)

Number	Loading	Description
27	60	I like to keep good control over my mental processes
19	−45	I want to inhibit my desire to see any formidable militaristic power that actively threatens the U.S.A. destroyed
13	32	I like to know a sure place to turn for help
32	29	I want to get a good reputation among those in my profession who can help my advance
17	−29	I want to inhibit my desire to satisfy my curiosity about everything around me
29	29	I want to become proficient in my chosen profession
26	24	I want to get a good reputation among those in my profession who can help my advance

for the loadings in 4 and 8 are such as have been generally accepted here as significant, hen we must seek a psychological hypothesis. Briefly our hypothesis is that the narcissism oadings in 8 (parental-protective) are merely part of the general positive loadings of the nating erg found therein in this rotation (Table 2). The loadings in 4, the sex erg, perhaps indicate that with these younger, adolescent, unmarried men the narcissistic component of sex has not yet separated out from the hetero-sexual interests proper. Such ndications by the present method, and we have not paused here to mention more than one or two, demonstrate its possibilities for investigating psychoanalytic theories objectively.

Our survey of meaning has left factor 6 uninterpreted. It has clear positive loadings above 25) in 26–32, 29, 14 and 10 (declining order) and negative (above 25) in 7, 2 and 6. It can immediately be perceived as a quite *psychologically* consistent pattern of self regard, with interest in religion and inhibition of sex expression. Thus it would have claims to be the self-sentiment, were not the loadings in 25, 26 and 27 much better in factor 9. This duality of self regard needs investigation, perhaps along the lines that the present factor is a super-ego pattern, never completely integrated into the conscious self

sentiment, as argued elsewhere (10). In general, the less satisfactorily matched factors are those of lesser total variance. Presumably the fringe of lesser variance factors picks up different statistically-subliminal factors in different populations.

In order that the reader may readily estimate both the adequacy of the simple structure obtained in dynamic material and the general soundness of the matching we have reproduced in Fig. 1 the projections of two factors from Table 2 above, along with the pro-

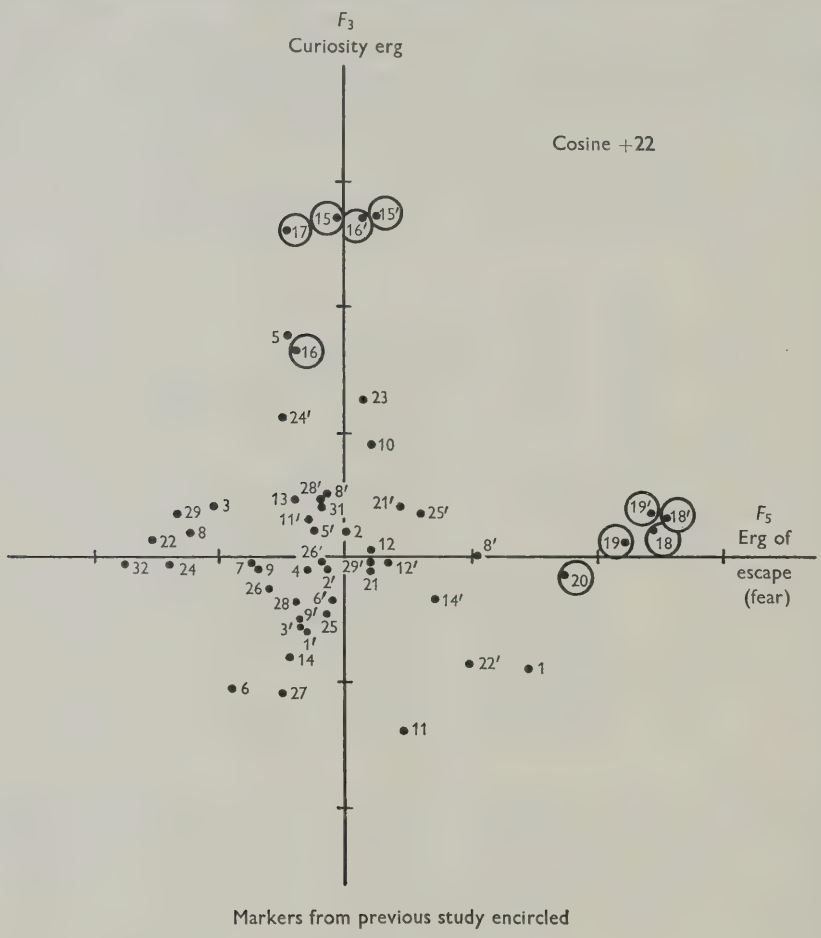

Fig. 1.

jections of the same variables obtained in the previous study. The numbers represent attitudes defined as in Table 2 and *primes* are added for the previous study values.

It will be noticed that where the same attitudes were used in both studies they fall in the same quadrants of the diagram and have the same relative significance of loading.

V. Summary and view of further research

A factorization of 30 attitudes, each measured by a pool of three objective sets of responses, and each pair or trio of attitudes chosen to represent an hypothesized ergic structure indicated in a previous exploratory study, yielded 9 factors. These when independently rotated achieved the resolution of a true simple structure, and it was then found that 6 of

them confirmed well and 2 tolerably the ergic patterns previously found. The more and less confirmed patterns are: escape, gregariousness, self-assertion, mating, appeal, and curiosity on the one hand and parental protectiveness and the self sentiment on the other. A factor contingently concluded to be super-ego structure also appeared here for the first time (except in previous general personality ratings and questionnaire factor *G* (4,12)), with small variance. Distinctive to this study are negative loadings of the protective erg in appeal attitudes and positive loadings in sex. The latter can be resolved alternatively by having mating, and parental factors distinct but positively correlated, as found independently elsewhere (11,14). The independent narcissism factor disappeared, but narcissistic attitudes had their main loadings in the mating erg in a way not found in the other study, with older and married men (11).

With this assurance of the profitableness of the factor analytic approach in revealing dynamic trait unities, at any rate of an ergic kind, and with this structuring of our knowledge about the nature of the main array of ergic patterns in the field, it would seem worthwhile to design more intensive studies restricted to one or two ergs at a time, to determine the patterns more thoroughly. The aim should be both to extend the examination to other attitudes in which it is suspected that a given erg obtains expression and also to find some new attitudes, symptoms, or even physiological measures, in which the defined erg is still more purely expressed than in the present most highly loaded 'markers'.

Although much remains to be done in terms of exactly delineating the patterns of ergs in standard sets of variables, this discussion needs to take account of further research aims which involve consideration of the whole theory of ergs. As indicated in several discussions of this line of research (3,6,10,11) the term *erg* has been adopted, instead of propensity, not only because of semantic clarity and convenience (brevity, easy modification into adjectival and adverbial forms, derivation from a Greek root indicating a source of energy, and freedom from the historical disabilities and prejudices associated with the vulnerable terms 'instinct', 'drive', 'propensity', etc.) but also because the new term is associated with new and operational methods.

Erg, unlike propensity, is an operational concept, meaning, in the first place *an independent pattern found by the operation of simple structure factor analysis among a set of measured dynamic variables.* Now the general theory of factor analysis would lead us to expect that *factors* in personality data would *not* be *partly* environmental and *partly* hereditary in origin but wholly one or the other, as in the notions of 'constitutional' and 'environmental mould' source traits (3,9) (clusters, or 'surface traits' are more likely to be partly environmental and partly constitutional). The fact that the common essence in any one of the present discovered patterns is an emotional quality and a single conative goal, as well as the similarity of these emotions and goals to the instinct patterns of the higher mammals, carries a strong presumption that these particular patterns are innate ones. Consequently, though nature-nurture research by twin studies, etc., is now essential to clearing up the matter, we have already ventured to define an erg hypothetically as follows: 'An innate psycho-physical disposition which permits its possessor to acquire reactivity (attention, recognition) to certain classes of objects more readily than others, to experience a specific emotion in regard to them, and to start on a course of action which ceases more completely at a certain specific goal activity than at any other.' ((10), p. 199; cf. (29).)

However, between the ergic patterns as here discovered and the above definition there intervene more research steps than that of the above investigation of innateness alone.

Our present cross-sectional analysis requires, indeed, to be followed by a longitudinal study of the relations among the same pattern elements, to demonstrate the functional connexions of perception, emotional reaction, goal striving and specificity of goal among them.

Before sketching this third step let us remind ourselves that the present patterns are presumably environmentally influenced to the extent that they depict the acquired reactions (attitudes) by means of which a given erg has typically come to be expressed in our culture. Accordingly we have suggested that this should be called the *displaced ergic pattern* (15) or culturally-invested erg. Research of a different kind would be necessary to dissect from this the innately preferred response patterns. However, the displaced ergic pattern can quite correctly be used as the basis of measurement for twin and other genetic studies. For to say that the displaced ergic pattern is partly environmentally determined is not to say that the strength of the *factor* is environmentally determined in the least.

The issue here is one which has been a constant source of confusion also in discussions on heredity in the abilities. The ability to spell may be wholly hereditarily determined, though all spelling is learnt and the factor of spelling ability shows itself in different patterns in different languages and cultures. In the present case part of the variance in the strength of *each of the attitudes* involved in the ergic factor pattern is derived from environmental sources, but these environmental sources are different for each attitude (or, at least, most attitudes) whereas the ergic factor is alone common. If the estimate of the factor is made on these *common* variances, omitting the specifics, the factor would be wholly innate.

The derivation of the innately preferred response pattern from the displaced ergic pattern is probably going to be attacked through the study of relative rigidities, taking the discovered common *goal* of the attitudes in the pattern as the undisplaced point of reference. Demonstration of the longitudinal pattern, the sequence of preferred perception, emotion and goal, as being linked in a single ergic pattern calls for statistical analysis to demonstrate invariable sequences, and the correlation coefficient can again be used here in a special form of factor analysis (13). But these operational clarifications of the ergic concept are for the future.

A brief reference must be made to the mystery of why the hypothesized sentiment structures have failed, with one exception, to be substantiated by any of these three dynamic factorizations. Although sentiments might not be expected to vary in strength sufficiently from day to day to be caught by *P*-technique, the argument that they should vary in habituation, according to the impress of social institutions, from person to person (thus appearing in *R*-technique), has no obvious flaw. Presumably that variance is yet to be discovered by more careful research with larger populations and improved objective measures. The outstanding research need to-day, in view of the demonstrated general effectiveness of the analytical procedure, is for such improved reliabilities and validities in the substrata of objective motivation measures.

Meanwhile, the self sentiment stands reasonably clearly demonstrated and certainly clearly distinguished on the one hand from the self-assertive erg, with which it has often been confused in the talk of 'ego-involvement' and, on the other, from the super-ego, as, in younger populations, a structure still only partly integrated with the self sentiment. In view of the recent swing of interest toward a more definite understanding of the self concept and the role of the self sentiment in dynamic regulation (10,17,22,25), this demon-

stration of a methodology for obtaining more positive information on its structure, its subsidiations and its relation to primary ergs may be useful.

Ergic analysis has its greatest potential practical application in clinical and social psychology. In social psychology the information it adds, in attitude and opinion measurement, about the ultimate interests behind the attitudes, should give an entirely new depth of resources for calculating predictions of attitude change. In clinical psychology ergic analysis presents a quantitative psychoanalysis, i.e. it reveals for what 'instinctual' ends a given attitude or symptom is at work. Thus if we consider, say, the specification equation for attitude no. 8, we obtain from Table 2 (neglecting all insignificant loadings, i.e. below 0·20):

$$I = \text{strength of interest in 'I like to take an active part in athletics'} = 0{\cdot}49\,E_1 - 0{\cdot}25\,E_5 + \text{specific variance.}$$

This indicates a substantial positive satisfaction to gregariousness and a slight negative satisfaction to the desire for personal safety. Again, the equation for no. 7, 'I like sex appeal in a woman', takes the values:

$$I = 0{\cdot}49\,E_4 - 0{\cdot}42\,S_6 + \text{specific variance,}$$

where E_4 is the individual's strength in the mating erg and S_6 is super-ego strength. These measures indicate a fairly strong conflict between sex interests and the super-ego. Other quantitative statements of interest roots and of conflicts involved—some considerable interest in various applied fields—can be similarly abstracted from Table 2. Naturally these are *R*-technique values, applying to the average case, not *P*-technique loadings giving dynamic structure for a unique individual. Applications in clinical and social directions, however, would be premature without extension of this basic research.

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THE BASIS OF PERCEPTUAL ANTICIPATION IN TRACKING

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I. *Introduction* (p. 295). II. *Speed anticipation in two-pointer matching* (pp. 296–297). III. *Course anticipation in two-pointer matching* (pp. 297–299). IV. *Number of events versus time in prediction* (pp. 299–300). V. *Visual 'stimulus' information in one-pointer balancing* (p. 300). VI. *Visual control information in one-pointer balancing* (pp. 300–301). VII. *Matching and balancing* (pp. 301–302). VIII. *Summary* (p. 302). *Reference* (p. 302).

I. Introduction

This paper follows on directly from a previous paper (Poulton, 1952). It was there shown that tracking was more accurate with a two-pointer display than with a one-pointer display; and that increased accuracy was associated with increased anticipation. In addition, a diagram of the experimental apparatus, and a definition of perceptual anticipation, were given. The present paper describes experiments which were designed to discover the information which was used in anticipating, and to determine the principal deficiencies of the one-pointer display.

In anticipating with a two-pointer display, the subject has two relevant sources of information available to him, only one of which is available with a one-pointer display while he is responding. The unique source is the velocity, and possibly also the acceleration, of the 'stimulus' pointer, which he can see directly. From this he can predict the position of the pointer one response time later, and thus achieve approximate alinement at this time. The use of this immediately perceived information will be called speed anticipation. Its experimental investigation is described in § II.

The source of information common to both types of display derives from the fact that after tracking for a while with any predictable course, the subject will have learnt its general characteristics. For example, if the course is harmonic, he will know its maximum amplitude of movement, its approximate mean amplitude, possibly the order of large and small amplitude movements, and also about the timing of the various components. The use of this remembered information will be called course anticipation. Its experimental investigation is described in § III.

As compared with two pointers, a one-pointer display has certain disadvantages. First, as already mentioned, the subject is not given direct visual information about the speed of the 'stimulus' movement while he is responding. He is only shown changes in the resultant error. A second disadvantage is the converse of this: the display does not give him direct visual information about his control movements. Finally, as a result of these two deficiencies, the subject cannot relate his control movements directly to the 'stimulus' movement.

(It will be seen that each of the first two deficiencies is necessarily associated with the third. Nevertheless, this third deficiency may be important in its own right. For when a part of a complete configuration is removed, the whole may lose certain configurational properties in addition to the individual properties of the part lost.) An experiment comparing these three deficiencies is described in §§ V–VII.

II. Speed anticipation in two-pointer matching

Experiments were carried out to determine whether the subject could use the immediate visual cues of the velocity, and possibly also the acceleration, of the 'stimulus' pointer in anticipating with a two-pointer display. He was given glimpses of the display of various lengths between constant length 'blackout' periods. This was achieved by standing a screened rotating transparent disk immediately in front of the display, and placing segments of black paper over it. The size of the clear segment and the speed of rotation were both adjusted.

The length of the glimpses was varied between 0·7 sec. and 1·5 msec. With glimpses of 0·7 sec., twelve subjects all reported that they could see the speed at which the 'stimulus' pointer was moving during the glimpses. Most reported that they could also see the pointer changing speed. Whereas with glimpses of 1·5 msec., none reported that they could see the speed of movement, although one said he could see the direction of movement. The complete results are given in Fig. 1.

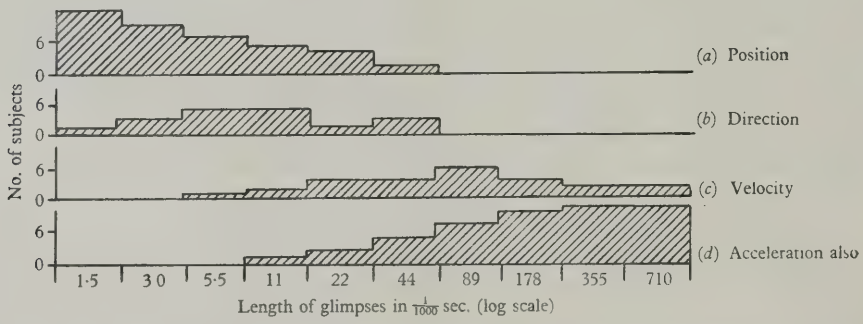

Fig. 1. Speed cues reported with glimpses of various lengths. A complex harmonic course and 1·0 sec. 'blackouts' were used. (*a*) Position of pointer only. (*b*) Direction of movement but not speed. (*c*) Speed but not acceleration. (*d*) Acceleration as well as speed.

The twelve subjects were all psychological research workers or assistants, aged between 25 and 34 years. They were tested separately. Each was presented with a single pointer moving in a complex harmonic course, and had merely to give judgements upon the speed cues. He was allowed as many glimpses as he liked before deciding, each glimpse being separated from the next by 1·0 sec. Half the subjects started with increasing lengths of glimpse, and finished with decreasing lengths. The other half started with decreasing lengths.

Despite the variability of judgements between one subject and another, the speed cues perceived varied directly with the length of the glimpses. With each length of glimpse, each of twelve further subjects was instructed to keep a response pointer in line with the 'stimulus' pointer, using a positional control. In order to do this during the 'blackout' periods when neither pointer was visible, he had to anticipate the movements of the 'stimulus' pointer.

In anticipating, the subject could use both the information which he had acquired previously during the experiment about the general characteristics of the course, and also the visual information presented at each glimpse. By using a Latin square design, it was possible to hold the general information constant, while varying the visual cues. The

mean size of the error over twelve half-minute periods has been plotted against the duration of the glimpses in Fig. 2.

Comparing Figs. 1 and 2 it will be seen that the major reduction in tracking error with increasing lengths of glimpses, occurred over the range of glimpse lengths where speed cues were becoming available. (The interpretation of this finding is complicated by the fact that as the length of the glimpses was increased, the percentage of the total time for which the display was 'blacked out' was reduced from 99·99 to 58. However, the first 8% reduction in the time 'blacked out', from 99·99 to 92%, was associated with an overall reduction in mean error of 16, from 55 to 39, for it was over this range of glimpse lengths that speed cues became available; whereas the final reduction of 18% in the time 'blacked out' from 74 to 58% was associated with an error reduction of only 1·5, from 33·5 to 32·0, as against 3·8 at the 5% point. For here there was little change in the availability of speed cues. Thus most of the reduction in mean error was associated

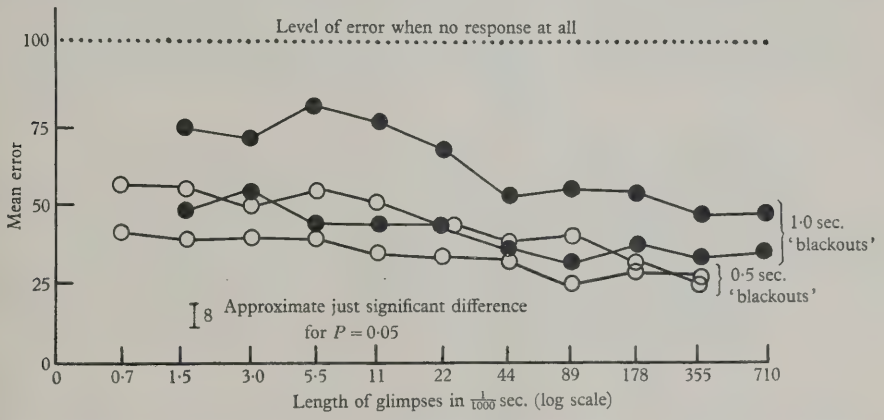

Fig. 2. Mean error in two-pointer tracking with glimpses of various lengths. This is shown separately for two 'stimulus' courses and 'blackout' lengths. Thin lines: simple harmonic course of periodicity 10 cyc./min. Thick lines: complex harmonic course, produced by three equal-sized harmonic displacements of speeds 5, 7 and 10 cyc./min. (The judgements shown in Fig. 1 were made upon this latter course.) ○, 0·5 sec. 'blackout' length; ●, 1·0 sec. 'blackout' length. For 5% significance, differences must be about 8. The horizontal dotted line gives the level of error if the subject had not responded at all. This has been taken as 100.

directly with the availability of speed cues, and not with the reduction in the percentage of the total time for which the display was 'blacked out'.)

In this experiment the subject was clearly using speed cues, when they were available, in order to anticipate during the 'blackout' periods. In normal two-pointer matching they are the only source of information which the subject can use in anticipating in the early stages of practice, before he has learnt the characteristics of the course.

III. Course anticipation in two-pointer matching

An experiment was designed to study course anticipation with a two-pointer display, under conditions which greatly reduced the usefulness of speed cues. When the subject has to predict the position of a 'stimulus' pointer moving with varying speed, further and further into the future, the present speed of the pointer becomes of less and less value to him. For it will have changed, and may even have reversed its direction. If his prediction

is to be reasonably accurate, he is therefore forced to depend more and more upon his previous knowledge of the 'stimulus' course.

The subject was instructed to make a single response movement in time with a bang produced by a hammer. This movement had to leave the response pointer at the anticipated position of the 'stimulus' pointer, when a bell sounded a constant 0·5, 1·5, or 3·5 sec. later. At the next bang, which followed 0·5 sec. after the bell, he had to make his next anticipatory movement, and so on. In order to move as the bang occurred (a 'premature reaction' in classical reaction time terminology), he had to initiate his response about 0·25 sec. after hearing the bell. Thus the subject had to respond discretely once every 1·0, 2·0 or 4·0 sec., predicting the position of the 'stimulus' pointer immediately before

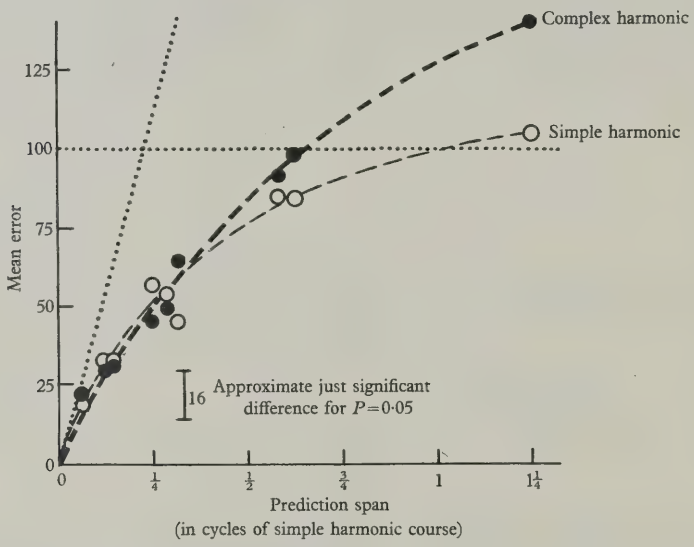

Fig. 3. Mean prediction error for different sizes of the span of the 'stimulus' course. ○ and thin broken line: simple harmonic course; ● and thick broken line: complex harmonic course, produced by three equal sized displacements with speed ratios 5 : 7 : 10. For 5% significance, differences must be about 16. The almost vertical dotted line gives the approximate 'following' error, i.e. the approximate error if the position of the 'stimulus' pointer at the start of the prediction span had been taken consistently as its position at the end. The horizontal dotted line gives the mean 'no response' error, i.e. the mean error if the mid-position of the display screen had been taken consistently as the 'stimulus' pointer's position at the end of the prediction span. This level of error has been taken as 100. The curves shown in broken lines have been drawn in freehand, to give an indication of the general trend.

his next response. At different times, three cam speeds of both the simple and complex harmonic course were presented, the conditions being arranged in Latin square form with twelve new subjects. In Fig. 3, the mean error per prediction has been plotted against the span of the 'stimulus' course at the end of which prediction had to be correct.

It will be seen that the subject was anticipating more accurately than by chance up to about three-quarters of a cycle ahead with the simple harmonic course, and up to about half a cycle ahead with the complex course. In anticipating with spans of this length, he could not have based his decisions simply upon the speed of the 'stimulus' pointer. For after half a cycle the 'stimulus' pointer would generally have been located upon the opposite side of the mid-position of the display screen, and have been moving in the

opposite direction with a different acceleration. He must have used his acquired knowledge of the characteristics of the 'stimulus' course, perhaps combined with the immediately presented information.

Although speed anticipation is more important than course anticipation in normal two-pointer matching in the early stages of practice, it is clear that a knowledge of the course must be an element in all perceptual anticipation. For in anticipating at all, the subject is making certain assumptions about the course, e.g. that it does not contain so many unpredictable discontinuities of position or velocity that anticipation is useless. In two-pointer matching, course anticipation becomes particularly important at fast cam speeds. For the faster the cam producing the course, the greater the course span which passes during the subject's reaction time delay, and consequently the more necessary it is for him to use his knowledge of the course to modify his speed anticipation (see § IV below).

In contrast, in one-pointer balancing, the slowly acquired knowledge of the course must be the principal source of information used in anticipating. For the subject is deprived of direct visual 'stimulus' speed cues while he is responding. It is presumably for this reason that anticipation normally occurs so gradually with one-pointer displays (Poulton, 1952).

IV. Number of events versus time in prediction

In the above experiment the subject had to predict ahead for different lengths of time, with each of three course speeds which were in the ratio of 1 : 2 : 4. Approximately the same amount of the 'stimulus' course could thus pass during a long prediction time with a slow course speed, as during a short time with a fast speed. The relationships of amount of course passing, time passing, and speed of course, are shown in Table 1 for the simple and complex harmonic courses combined.

Table 1. *Mean prediction error for simple and harmonic courses combined*

		Prediction span in seconds			5% point for differences
		0·75	1·75	3·75	
Approximate prediction span in cycles of the simple harmonic course	0·06	20*a*	—	—	10
	0·14	31*b*	32*a*	—	
	0·28	52*d*	52*b*	53*a*	
	0·60	—	87*d*	89*b*	
	1·25	—	—	117*d*	

This has been classified in three ways: in terms of the approximate amount of the 'stimulus' course within the prediction span, in terms of the temporal length of the prediction span, and in terms of the speed of the cam producing the 'stimulus' course. The letters *a*, *b* and *d* refer to the cam speeds. The simple harmonic speed course was 5 cyc./min. The corresponding complex harmonic course was produced by three equal-sized displacements with periodicities of $2\frac{1}{2}$, $3\frac{1}{2}$ and 5 cyc./min. Speed *b* was twice that of speed *a*, speed *d* four times that of speed *a*. The level of error which would have occurred if the subject had not responded at all has been taken as 100.

It will be seen from Table 1 that, whereas there are significant differences within each time span and cam speed, there is none within spans expressed in terms of the approximate length of the 'stimulus' course. It was for this reason that in Fig. 3 the mean error was plotted against length in cycles of the prediction span. For amount of course was clearly the principal determinant of accuracy. (Fig. 3 shows the degree of approximation in prediction spans made in constructing Table 1.)

The theoretical importance of this finding derives from the fact of the subject's reaction time delay, a time span the statistical distribution of which is constant for any given complexity of choice of response. The only way in which the subject can compensate for this inevitable delay in responding is by anticipating. It follows from the above finding that for any given temporal length of the delay, the faster events are occurring the less accurate the subject's perceptual anticipation at the end of the delay will inevitably be.

V. Visual 'stimulus' information in one-pointer balancing

An experiment was designed to determine the nature of the difficulties in tracking with a one-pointer display. The subject's task was to counteract a harmonic 'stimulus' movement, using a positional control, and thus to keep a single pointer on a fixed line. In different experimental periods he was presented with one or two additional pointers, which moved in the same dimension on either side of the error pointer. These showed him the 'stimulus' movement and/or his response movements. The effect of the additional pointers upon his performance was noted. A Latin square design was used, with twenty-four new subjects.

It was found that the mean size of the error over 48 half-minute periods when tracking with the single error display pointer was 41·1. With an additional 'stimulus' pointer it was reduced to 36·7, a difference of 4·4 as against 3·1 at the 5% point. (The results for a simple and more complex harmonic course have been combined, and the level of error which would have occurred if the subject had not responded at all has been taken as 100. The corresponding difference shown in Fig. 4 below is not significant, as it is only for twelve out of the twenty-four subjects.)

With the error pointer alone, the subject could see the size, direction, and rate of change of his error. The additional 'stimulus' pointer merely gave him direct visual information about the 'stimulus' movement. That improvement followed the presentation of these additional cues shows first that the subject could use these cues. This corresponds to the result in two-pointer matching discussed in § II above. It shows, secondly, that the absence of these cues is a disadvantage of a one-pointer display as compared with a two-pointer matching display.

VI. Visual control information in one-pointer balancing

It was also found that the mean error with a single additional response pointer was 39·8. This was a reduction of only 1·3 as compared with tracking with the error pointer alone, a difference which was not significant. The response and error pointers lay side by side and were both linked to the positional control so that a given control movement moved both of them the same distance. Thus the response pointer gave the subject visual information as to the effect of his control movements upon the error pointer.

There are two possible ways in which this additional information might have assisted the subject. First, it made it possible for him to learn quickly the extent of control movement required to correct a visible error of any given size. The present results suggest that this control-display relation must have been learnt during the standard practice period which preceded the experiment; and that the continuous visual check upon control movements which the response pointer permitted during the experiment was unnecessary.

Secondly, from the adjacent error and response pointers, the subject might have been able to extract information about the 'stimulus' movement. Had he been able to do so, he could presumably have used it, as he could use it when it was presented directly (see § V above). The present result suggests that in this respect the visible response pointer was of no greater use to him than the immediate sensory cues provided by his control movements.

VII. MATCHING AND BALANCING

In one half of this experiment (performed by twelve subjects), one-pointer balancing with a single error pointer and with additional pointers was compared directly with two-pointer matching. Fig. 4 shows the mean size of the error over twenty-four half-minute periods under the different display conditions. It will be seen that the improvement in performance in two-pointer matching as compared with one-pointer balancing, was more than twice as great as the sum of the improvements with an additional 'stimulus' pointer, and with an additional response pointer.

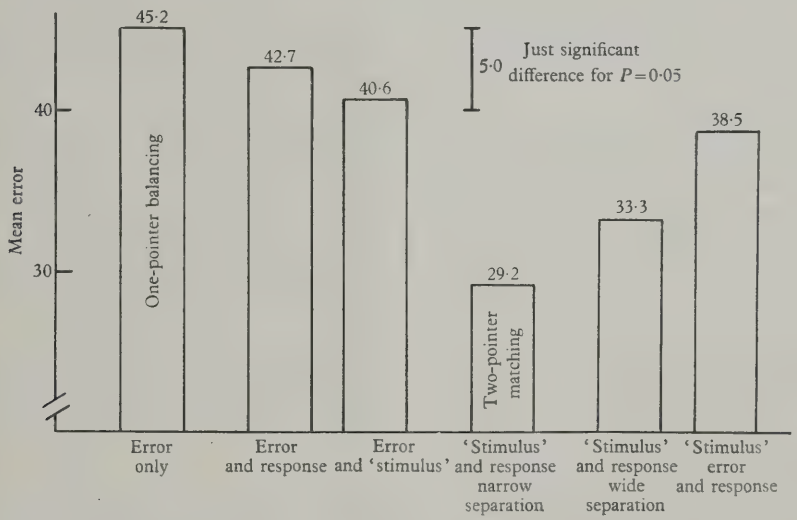

Fig. 4. Mean error in one-pointer balancing with and without additional 'stimulus' and response pointers, and in two-pointer matching. The results are from twelve subjects, each performing with a simple and a more complex harmonic course. For 5% significance, differences must be 5·0 or greater. The level of error which would have occurred if the subject had not responded at all, has been taken as 100.

The level of the two-pointer matching performance clearly could not have been predicted from the improvements in one-pointer balancing with the single additional pointers. The principal deficiency of the one-pointer balancing display, as compared with the two-pointer matching display, must therefore have been that the subject could not relate his control movements directly to the 'stimulus' movement. Two-pointer matching appears to be essentially a matching of the 'stimulus' movement by control movements, with continuous knowledge of results. Unless the tracking display makes this possible, performance will be a great deal less accurate.

When tracking with visible error and 'stimulus' pointers, the subject was in fact supplied with all the information which he had in two-pointer matching. For his responding arm provided him with sensory cues as to his control movements, and he had

learnt the control-display relation (see § VI above). That under these conditions his performance was so much less accurate than in two-pointer matching, suggests that in order for control movements to be related to 'stimulus' movement, both must be presented together in the same sense modality.

Further, for greatest accuracy, the tracking display must present the 'stimulus' and response movements in such a way that an immediate comparison is possible. In the same half of the present experiment it was found that a simple increase in the lateral separation of the 'stimulus' and response pointers by 3·5 cm. produced a deterioration which was nearly significant. And placing an error pointer between the two widely separated 'stimulus' and response pointers, was found to give a further deterioration which was significant at the 5% level. This is also shown in Fig. 4.

VIII. Summary

1. In anticipating with a two-pointer display, speed cues can be used (speed anticipation). In the early stages of practice, before the characteristics of the course are known, they are the only available source of information upon which anticipation can be based.

2. Once the characteristics of the course are known, they can be used in anticipating with both two-pointer and one-pointer displays (course anticipation). Certain course characteristics are in fact assumed in speed anticipation. Course anticipation is particularly important in one-pointer balancing, in which 'stimulus' speed cues are not normally available; and in two-pointer matching with fast 'stimulus' cam speeds, when the 'stimulus' speed changes so rapidly that it is of little predictive value by itself.

3. The accuracy of perceptual anticipation depends principally upon the number of the events at the end of which prediction has to be made. The time over which the events are spread is of only secondary importance.

4. The principal deficiency of a one-pointer display, as compared with two pointers, is that control movements cannot be apprehended directly in relation to the 'stimulus' movement. For the greatest accuracy in tracking, control and 'stimulus' movements must be presented for direct comparison in the same sense modality, and in such a way that an immediate comparison is possible.

5. The simple absence of 'stimulus' speed cues in one-pointer balancing was found to be a significant but less serious handicap.

6. The simple absence of a visual representation of the control movements in one-pointer balancing was not found to be a significant handicap.

I am greatly indebted to Prof. Sir Frederic Bartlett, F.R.S., for presenting the problems and theoretical background out of which these experiments have developed. Also to Dr N. H. Mackworth and Dr W. E. Hick for their help, and to members of the research staff of the Psychological Laboratory at Cambridge for acting as subjects. The majority of the experimental subjects were supplied by the Royal Navy. I carried out the experiments while working as a member of the Medical Research Council Applied Psychology Research Unit at Cambridge.

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CRITICAL NOTICE

By L. S. HEARNSHAW

Thinking. An Introduction to its Experimental Psychology. By George Humphrey. London: Methuen. 1951. Pp. xii–332. 21*s*.

In this 'strictly introductory volume' on the experimental psychology of thinking Prof. Humphrey, whose earlier work, *The Nature of Learning*, has deservedly won a high reputation, sets out to help psychologists planning research into 'The all-important subject of the intellectual processes of human beings'. He can rest assured that he has helped them in generous measure in writing a book of the highest scholarly standard, and by giving them a guide upon which they can, and no doubt will, rely for many years to come. Within the rather restricted field which Humphrey has marked out for himself there is nothing comparable in the English language. Even on the early work of the Würzburgers Humphrey's meticulous account and penetrating yet sympathetic criticism gives us a much clearer idea of the Würzburg contribution than the almost contemporary lectures of Titchener to which English readers have hitherto mainly had to refer. If, as Boring asserts, 'the gift of professional maturity comes only to the psychologist who knows the history of his science' Humphrey's book must be regarded as fulfilling an important service by reminding us of persistent problems and of the need for historic orientation. Both in style and in scholarly thoroughness it is a book worthy of the traditions of the ancient university from which it emanates.

It is, however, important for the prospective reader to recognize that what Humphrey has produced is essentially an historical work. It is not a laboratory manual. It does not discuss systematically the methodology or the technical difficulties of experimentation upon thinking. There is nowhere any indication that Humphrey has carried out any experiments himself, though, of course, he may modestly have refrained from mentioning them. The book smells heavily of the lamp rather than of the laboratory. Its methods are those of the scholar rather than the experimental scientist, and one would have thought that the word 'historical' prefaced to the subtitle, 'an introduction to its experimental psychology', would have described its contents more adequately and possibly obviated misconceptions.

Humphrey devotes the first chapter of his book to Association 'with which an account of the experimental psychology of thinking logically begins'. Association is the primary doctrine of thinking from which psychologists have somewhat unsuccessfully attempted to free themselves. Humphrey conceives this revolt as taking place successively on various planes; first on the purely analytic plane, as represented by Bradley, Stout, Ward and Adamson; then on the experimental-introspective, as represented by the Würzburg psychologists and Selz; finally, on the experimental-objective, as represented by more recent protests against 'connectionism' and Pavlovian conditioning, which in many respects, though not in all, is the modern form of the classical theory of Association. This theme of revolt sets the main pattern of Humphrey's book. After dealing briefly in the first chapter with the analytic phase of the revolt, he passes in turn to the Würzburg work, Selz, and Gestalt psychology; considers evidence for and against peripheral theories which identify thought with motor reactions or with language; and concludes with a chapter on generalization.

The detailed account of experimental work which Humphrey provides is so valuable a contribution to psychological history that the framework within which it is set may seem a matter of relative unimportance. However, if we are to evaluate, and not merely describe, the work of the thought psychologists an account of the background against which their work was carried out becomes highly relevant. In the tenuous realm of thinking, experimental findings and philosophical preconceptions are not easy to distinguish. If we unduly simplify the background of the early experimentalists their preconceptions may well be mistaken for genuine experimental findings. This particularly applies to introspective reports. The data of consciousness are often, when observed in the laboratory, as vague and unstructured as 'inkblots', and self-observation runs the risk of being no more than a kind of inner 'projection' of preconceived notions.

There are grounds for asserting that the main plot of the drama as sketched by Humphrey is distinctly over-simplified. The place of association in the history of European psychology can easily be exaggerated. The 'philosophia perennis', against certain aspects of which the doctrine of association was itself a revolt, formed a never broken thread in the history of modern thought. Aristotle may have formulated the laws

of association, but his complex analysis of intellectual processes is far removed from associationism, and had a profound and lasting influence on most subsequent accounts of thinking. Even Bain in the hey-day of associationism added a long appendix by Grote on the psychology of Aristotle to the third edition of his *The Senses and the Intellect*. We may well point, too, to Aristotle's statement that intellect does not provide its own drive ('διάνοια αὐτὴ οὐθὲν κινεῖ', *Nich. Eth.* VI, 2) as an early statement of the necessity for motivation. Associationism was indeed never more than one among many other influences on the early German experimentalists. It was for them a largely alien influence, which had to compete with a number of indigenous and powerful currents, Aristotelian, Kantian, Hegelian, phenomenological, etc. Indeed not only was the intellectual environment in which experimental psychology arose in Germany heavily charged with philosophy, but many of the leading German psychologists were themselves considerable writers on philosophical subjects, and this applies in particular to Külpe himself with whom the experimental psychology of thinking takes its rise and inspiration. Külpe's *Einleitung in die Philosophie* first appeared in 1895, and after his death posthumous editions were edited by Messer, another of the Würzburg psychologists. Külpe's *Die Philosophie der Gegenwart in Deutschland* appeared in 1902 just at the time of the early Würzburg work. Nor in view of some of the Würzburg findings should we overlook Külpe's earlier article (1888) *Die Lehre vom Willen in der neueren Philosophie*. It may perhaps be suggested that if, viewed negatively, thought psychology was a revolt against associationism, it was, viewed positively, an attempt to cast an experimental blessing on rival doctrines of even older vintage. And Humphrey may be criticized for giving us only one side of the picture.

One influence, partly because of its importance, and partly because it is wholly ignored by Humphrey, requires special mention. Just before the turn of the century there was on the continent of Europe a remarkable revival of Aristotelian and scholastic modes of thought. The standard histories of psychology recognize in particular the influence of Brentano, and Titchener in his half-finished *Systematic Psychology* makes him the principal rival of the new Wundtian experimentalism. Even in this country the influence of Brentano was acknowledged by Ward and Stout, and we are assured by Flügel, who should know, that he was a directing influence on Spearman. His impact on German philosophy and psychology, as Boring and others have pointed out, was considerable, and it is probably not rash to regard him as one of the major determinants of the Würzburg work. Messer at least among the Würzburgers was an open adherent of the Brentano viewpoint.

But neo-scholasticism was not confined to the immediate disciples of Brentano. In 1879, after a long period of decay—though not extinction (Leibniz, for one, was markedly influenced by scholastic philosophy)—Pope Leo XIII in the encyclical *Aeterni Patris* restored Thomism to its place as the official philosophy of the Roman church, while insisting that account must be taken of progress in knowledge. Among the results that followed this encyclical was the establishment of a higher institute of philosophy at the University of Louvain (1890), which was followed in 1901 by an institute of experimental psychology. The revived Thomism soon became influential in psychology. Spearman, for instance, more than once 'harks back to the usage of the mediaeval schoolmen', and asserts the claims of scholasticism.

That this neo-scholasticism was a major influence upon the thought psychologists is not only implied by Wundt in his criticism of the Würzburg school, but seems probable from the prominent part subsequently played by Catholic psychologists in the new movement—by T. V. Moore in America, Aveling in England and Lindworsky in Prague. The distinction between sense and intellect was a key doctrine in scholastic philosophy. In part, this no doubt derives from the Aristotelian theory of an immaterial νοῦς 'separate, impassive and uncompounded', but it was elaborated and sharpened in the Thomistic system. The 'imageless thoughts' of the Würzburgers were soon assimilated to, if they did not actually derive from, the 'species intelligibiles' of Aquinas. Prominent too in Thomism is the doctrine of the will, and another indication of the impact of Thomism on experimental psychology is the fact that Michotte in Louvain and Aveling in London were soon experimenting on volition. How far, we would like to know, was the work of Ach, *Ueber die Willenstätigkeit und das Denken*, influenced by these scholastic concepts? Are his 'determining tendencies' derived from the scholastic will? These are intriguing questions which need answering before the work of the experimental-introspective period can be properly evaluated, before we can be reasonably confident that we are being served with genuine experimental findings rather than with philosophical concepts in an elaborate scientific disguise. It is to be hoped that before it is too late, and while workers from the early years of the century are still happily among us, someone who was linked with these movements and who knew the men can tell us the answers!

To his account and critique of the Würzburgers themselves Humphrey devotes no less than one hundred pages of his book, and a further eighteen are allotted to the massive contributions of Selz. Even Titchener writing in 1909, when Würzburg was the latest fashion, expresses 'a certain aversion to the scores of

closely packed and spottily printed pages of the *Archiv*'. The psychologist of to-day, whose German is rarely fluent, would find the task of ploughing through the original articles even more abhorrent, and his gratitude to Humphrey for his careful, detailed and scholarly summaries, together with his penetrating criticism, is correspondingly great. There is no doubt that Humphrey's judgement in leaving these chapters in their present form in spite of some pressure to delete them was entirely right. They constitute the most valuable part of the book, and are a contribution of major importance to the history of experimental psychology.

In spite of its technical flaws and methodological weakness, and in spite of the outdated framework of 'presentationism' within which it was conducted, Humphrey evaluates the Würzburg work very highly as 'the most massive, sustained and acute experimental attack on the problem of thought', as 'a milestone in the history of experimental psychology' and perhaps more significantly as 'a kind of preview of work which is central for modern experimental thinking'. He dismisses the Wundtian criticisms of the *Ausfrage* experiments as 'shallow and pretentious', and regards the contradictory results of Titchener as wholly unconvincing.

Probably not many present-day psychologists will feel able to follow Humphrey in this evaluation. The methodological inadequacies to which Humphrey more than once refers will seem seriously to undermine the conclusions of the Würzburg experiments and the contradictions between the Würzburg workers and workers elsewhere a direct consequence of these methodological deficiencies. As Humphrey suggests in his criticisms of Ach, the conclusions reached were not really experimental at all; they were derived from antecedent dogma. It is doubtful whether either of the two major findings of the Würzburgers, that thought is imageless, that thought is directive, was a genuine experimental finding. We may suspect that they both spring ultimately from philosophic sources; the former from the 'species intelligibiles' of the scholastics; and the latter in part from the scholastic doctrine of the will, and in part from the romantic, anti-rationalistic philosophies which stressed impulse and subordinated thinking to the 'hormic' side of the mind. It did not need the Würzburgers to show that thought was imageless or that thought was motivated. These doctrines were already widely held on analytic grounds. Ribot in his *L'Evolution des idées générales*, published in 1897 just prior to the Würzburg efflorescence, makes a quite clear distinction between ideas and images, and holds that the former can occur without the latter. In his *Manual of Psychology* (1898), to quote another work just prior to the Würzburg period, Stout, when writing on 'Trains of Ideas', refers to 'the conative tendency guiding the whole process'.

To point to the derivation of a doctrine does not, of course, necessarily invalidate it. The Würzburg conclusions may be perfectly correct. The crucial issue is one of methodology. 'Once the introspective method has been admitted as methodologically sound', writes Humphrey in answer to Wundt's criticism, 'it must be assumed to be applicable to all mental events'. Quite apart from the extreme dubiety of this assumption, it is precisely the methodological soundness of introspection that is suspect, and Humphrey should not merely have answered Wundt's criticisms of the use of introspection in certain circumstances, he should have endeavoured to answer our present-day far more radical doubts as to the status and value of introspection in general. For the fact is that the modern reader of the Würzburg work will for the most part remain quite unconvinced by the lengthy introspective analyses, and sceptical of conclusions attested only by introspection. By contrast, the occasional objectively assessable achievement (as for instance Bühler's memory experiments which Humphrey reports on pages 61 and 62 of his book) will seem refreshingly acceptable. Humphrey's critique does not go far enough to rehabilitate the Würzburg work; nor does it explain its mysterious cessation after Külpe's departure to Bonn in 1909. Why, if *Denkpsychologie* was such an important milestone, did it fade out as it did? Is it possible that Boring is right in asserting that 'the work of the school had failed in its positive purpose'? Is Ward's evaluation nearer the mark than Humphrey's when in *Psychological Principles* he describes the Würzburg output as a series of books and articles of inordinate length and uncertain value'.

This criticism of Humphrey's evaluation, however, is no reflexion on his wholly admirable exposition of the work of the Würzburg group, nor of the equally valuable summary of the thousand pages contained in Selz's two untranslated books on thinking. The work of Selz has been rarely referred to by English-speaking psychologists (apart from Spearman) and Humphrey's account, even if 'step-motherly', is a most useful introduction.

In the chapter which follows on 'The Gestalt theory of thought' Humphrey bases his exposition primarily on Köhler's fundamental work with chimpanzees, Duncker's monograph on problem-solving, and the studies of Ruger, Claparède, Bulbrook and Maier. The chapter makes only a passing reference to Wertheimer's *Productive Thinking* (1945), a book which provides by far the most stimulating and readable account of the Gestalt theory of thought at the human level, though it does not set forth any radical

changes of gestalt doctrine. Wertheimer's stress on the obstacles to productive thinking arising from a slavish adherence to mechanized habit was strikingly confirmed in Luchin's ingenious experiments on 'Mechanisation in problem solving' to which Humphrey might also have devoted more space than the very brief note appended to the chapter. One cannot but suspect that this chapter was finished with the first draft of the book before the war, and has since received only slight modification and revision. It is indeed a general criticism, which would have been more serious in a work which is not so predominantly historical, that the work of the last decade receives relatively scanty notice.

In his chapters on 'Thought and motor reactions' and 'Language and thought' Humphrey comes down in favour of a 'central' theory of thinking. 'By a central theory of thought is implied objectively and neurologically that the highest psychological activities are most properly described in terms of a central neural pattern, which is independent of the particularity of the motor outlet' (p. 262). Peripheral activities, whether they be motor responses or linguistic utterances, cannot wholly explain thinking. Nevertheless, motor responses are ubiquitous, and it was the great positive contribution of Titchener and the Cornell school, in Humphrey's opinion, to have shown the close relationship between thinking and muscular response. Humphrey reviews at some length the work of Jacobson, best known for his book *Progressive Relaxation*, and of G. L. Freeman, but he does not consider that they have proved thinking and muscular process to be identical, even if they have shown that thinking is always muscularly accompanied and ceases when all muscular tension is removed. The work of Lashley on animals, and the work of various investigators on the activities of the speech muscles during thinking, point against a peripheral theory. Humphrey suggests that the proprioceptive impulses resulting from muscular tension serve as a general reinforcement to increased cerebral activity.

The following chapter on language falls into three main parts. There is a discussion of the principal aspects of language utterance, evocation and reference; a brief account of aphasia, based mainly on the work of Head (1926) and Weisenburg and MacBride (1935); and a final section on the relations between thought and language. In some places the chapter suffers from having been based primarily on sources available prior to the war. For instance, language is regarded as a uniquely human activity (p. 217) and animal language is stated to be 'entirely material and emotional' (p. 235). No mention is made of the work of von Frisch on communication between bees, which, according to the zoologist, Pumphrey, 'has demonstrated beyond cavil that the returning worker-bee delivers to her colleague in the hive a message as concise and explicit as a naval signal'.

In discussing the reference function of language Humphrey stresses the viewpoint already familiar to readers of this *Journal* that 'there is no problem of meaning'. By this he implies that there is no separate or unique problem of meaning at least for the psychologist. The so-called problem of meaning reduces without remainder to the general problems of perception, response and learning—particularly learning. There is much in such a reductive theory of meaning that is attractive. The corner stone of the anti-scientific, interpretative or 'geisteswissenschaftliche' schools of psychology is the uniqueness of the meaning or reference function of mind. If meaning can be shown to be a pseudo-problem the path of psychology as a natural science is cleared of a formidable obstacle. Humphrey's theory, however, is confronted with a number of difficulties which up to the present he has far from demolished. It involves the assumption that we not only perceive, but image and think of objects directly, a form of naïve realism which has never withstood critical scrutiny. It asserts that the psychological problems of meaning are wholly accounted for by learning, which does not follow, even if meanings are invariably learned. Learning results in the growth of dispositions, and these are presumably structures whose psychological nature is by no means fully explained by the process of learning itself. Indeed, such a view would be hardly tenable, since learning leads to the establishment of dispositions of different kinds, to skills, for instance, and to knowledge. We learn *how* and we learn *that*. The structure of skills and the nature of the insight involved in knowing remain real problems for the psychologist even after he has exhaustively explored the processes by which they were acquired. The reductive disposal of meaning, though it holds out attractive prospects, remains far from accomplished, and Humphrey has not succeeded in removing the uneasy fear that this beguiling simplification may prove a mirage.

In the final chapter before his brief conclusion Humphrey deals with 'Generalization', and gives us accurate, concise and lucid summaries of the work of Külpe, Grunbaum, Fisher, Hull, English, Ach, Willwoll, and others. But it is a pity, even in a book that sets out to be merely introductory, that Aveling's work *On the Consciousness of the Universal and the Individual* (1912) is mentioned by name only and that the pioneering nature of his investigation and of the techniques he employed does not receive the recognition it deserves. Not only was Aveling the first psychologist to investigate concept formation experimentally, but he devised the method of associating nonsense words with conceptual groupings

which has played a prominent part in subsequent research in this field. Although his reliance on introspection dates his work, his research is a landmark in the history of research into generalization.

The main comment on this chapter, however, must refer not to its contents but to its relation to the remainder of the book. It comes as something of a shock to the reader to be transported back again to Külpe and the Würzburgers whom he has left several chapters before. The historical arrangement which Humphrey has more or less followed up to this point seems suddenly violated. And why should generalization, we wonder, be thus singled out for special treatment? Have the previous chapters excluded generalization? If so, what kinds of thinking have they been discussing? To these questions we can find no clear answers. If we were to list the kinds of problem being used to provoke thinking in the researches Humphrey describes, we should get a miscellaneous collection of brain-teasers, mostly artificial and often apparently trivial. Few have the refreshing reality of the samples of thinking which Wertheimer examines in *Productive Thinking*—from a girl describing her office to Einstein pondering on relativity. But the problem raised by this chapter is not so much the selection of specimens—whether dead or alive—but their classification. If there is any scheme of classification implicit in Humphrey's book, it is probably not unfair to say it is that of faculty psychology. Thinking is divorced from the 'lower' faculties of perception, memory and imagination; while the principal intellectual processes appear to be abstraction, generalization, judgement and reasoning. Factor analysis is mentioned only twice in brief footnotes, though the work of Spearman and his followers on the nature of intelligence might well have been considered to be of some relevance. Nor is there any systematic description of what Stern, in his *General Psychology*, calls 'the modes of appearance of thought'. Neither the regulative nor the creative and imaginative side of thinking is anywhere considered. We are left, then, with a series of chapters in roughly historical order in which thinking is defined rather broadly 'as what occurs when an organism, human or animal, meets, recognizes and solves a problem' (p. 311); and a final chapter in which generalization, 'the activity whereby an organism comes to effect a constant modification towards an invariable feature or set of features occurring in a variable context' (p. 315) is singled out for special treatment. The sharper focus in this final chapter brings out by contrast the relative want of focus in the earlier part of the book. The lack of any explicit classification of 'the modes of appearance of thought', or analysis of its factorial structure, leads to a blurring of outline which renders the net result of all this work very hard to define. When we have judged weights with Marbe, found associations with Messer, Watt or Ach, been asked by Bühler to comprehend the almost incomprehensible, filled in gaps with Selz, worked out problems with Duncker, relaxed with Jacobson, and been tongue-tied with Head—what have we really been doing? It is surprisingly hard to say!

One final, and indeed major, criticism remains. Experimental psychology began as the investigation of the mental processes of the normal, educated adult in the somewhat artificial conditions of the laboratory. Nearly every subsequent development in psychology has been a protest against the stultifying narrowness of this tradition. Humphrey, by excluding data from pathological, genetic, social, differential, comparative and psycho-analytic sources deliberately reinstates, and it may be thought tacitly approves, the old limitations. The fact that Humphrey must at times transgress his restrictive frontiers, and must call on Köhler's apes and Head's aphasics to help, where the normal adult fails, should have warned him of the unreality of his imposed limitations. Though so much less thorough and scholarly than Humphrey's *Thinking*, T. V. Moore's neglected *Cognitive Psychology* (1939) perhaps more adequately maps out the proper scope of a psychology of thought.

Human thinking is the most complex activity of the most complex of living organisms. A direct experimental attack upon thinking in its developed forms has, as Humphrey points out in his concluding summary, 'not brought us very far'. Possibly the road forward is by the very paths which Humphrey by-passed. The study of thinking ought not to be divorced from its birth in the animal mind, from its links with perception and learning, from its growth in childhood years, from its social functioning in the adult, from its primitive manifestations in myth and dream, and from its breakdown in brain-injured and schizophrenic patients. Experimental psychology is not an area, isolated from the rest of psychology, but a method to be used wherever possible. Nor can thinking be segregated from the personality and the social context of the thinker, as the experimentalists have tried to segregate it.

In deciding what to include and what not to include in his account of thinking Humphrey was confronted with a major decision. He decided to confine his study to experimental psychology in the older sense, and when taxed with his omissions he answers in his preface that 'what has been written is already long enough'. We can indeed sympathize with the prolonged gestation and immense travail which the thoroughness of Humphrey's book must have entailed. But the outcome of this decision is not only quantitative, but qualitative. It determines the whole character of the book and turns it towards

the past rather than to the future. Many lines of influence have in recent years converged to make the study of the central processes of thinking one of the growing points of psychology. In a book with the bold and simple title, *Thinking*, one was perhaps led to expect a review of these varied influences and a glimpse of the vistas which seem to open out for the future explorer. In this Humphrey has disappointed us. What he has given us instead is a historical survey of the highest excellence of one line of influence, which he has so meticulously charted that for generations to come psychologists who are interested in the history of their subject are quite certain to consult it. For the labour and critical acumen which have given us the most scholarly work produced by a British psychologist for some years Humphrey has earned our warmest gratitude.

PUBLICATIONS RECENTLY RECEIVED

Our Age of Unreason. By Franz Alexander, M.D., Philadelphia & New York: J. B. Lippincott Company. Revised edition. 1951. Pp. 338. $4.50.

This is a revised edition of a work that first appeared in 1942. It is an essay in applied psycho-analytical sociology by a distinguished psychiatrist. Unlike a great many works of this kind it makes a serious effort to do justice both to the sociological and psychological aspects of the facts it deals with. The point of view is on the whole Freudian. But though Prof. Alexander defends Freud against the criticisms which have been directed against him by such writers as Karen Horney and Erich Fromm, his own position differs in fundamental respects from that of Freud. He rejects the dichotomy between the life and death instincts and replaces it by another, namely the energy saving principle and the principle of surplus energy. The former accounts for adaptive behaviour, but also for the phenomena of regression; the latter is realized in reproduction but also in artistic, intellectual and moral creativeness. Aggressive behaviour is regarded as the result of frustration in the gratification of needs and not as a primary drive to hurt and destroy.

The link between sociology and psychology is found in a theory of social institutions as crystallized or solidified attitudes. Institutions, in other words, are the result of efforts by individuals at adaptation, but once formed they exercise a constraining or determining influence on succeeding generations. Since the institutional structure undergoes change, there may result a 'cultural lag', that is, a discrepancy between the new social structure and the attitudes which represent adjustments to conditions no longer prevailing. Subjectively, the discrepancy appears in the form of widespread feelings of discontent and insecurity, and is also, in certain conditions, expressed in delinquent behaviour and in psychoneuroses. The causation of psychoneuroses is to be sought in the individual, in the sense that they depend on a combination of hereditary or constitutional factors and unfavourable experiences during childhood. But their general spread is a sign, Prof. Alexander thinks, of social disintegration—a disturbance of the balance between the prevailing attitudes and the changing social structure. The conception of a cultural lag is due to Ogburn, but Dr Alexander gives it a broader application and stresses its psychological aspects. He uses it in an illuminating manner in an impressive survey of the emotional background of totalitarianism and democracy in their various forms.

His analysis of American conditions is of especial interest. The 'cultural lag' there takes the form of a discrepancy between the tradition of aggressive individualism and the worship of individual success, developed in the period of the frontier society and of an expanding economy, and the new social structure which is no longer compatible with unlimited individualism. Indicators of this lag are crime and mental disturbance. Both are the result of a conflict between competitive self-assertion and the need for security and dependence. The clinician, Dr Alexander tells us, is particularly struck with the frequency with which patients are seen to be driven by a relentless competitiveness and in whom analysis reveals a repressed desire for dependence, love and protection. The exceptional frequency and intensity of this type of conflict in the U.S.A. is to be explained historically and sociologically as due to the inconsistency of American social standards—the traditional one-sided worship of individual success in a complex interdependent society and the exaggerated emphasis of independence in times of great insecurity.

The book makes a powerful impression both by the wide range of facts which it covers and the skill with which the fundamental principles adopted by the author are utilized in interpreting them.

MORRIS GINSBERG

Personality and Conflict in Jamaica. By Madeline Kerr. Liverpool University Press. 1952. Pp. xii + 221. 15*s*. net.

Psychologists venturing into another culture sometimes merely wish to find out if a particular test will work there. Dr Kerr's aim lies at the opposite end of the scale, as she is interested in the kind of personality that has resulted from social conditions in Jamaica. The book is based on two years' fieldwork, whilst the author was Social Psychologist to the West Indian Social Survey.

At the outset the reader is vividly introduced into the atmosphere by a number of life-histories. Then, after a brief background sketch of the field centres, follow several chapters on development from birth to adolescence. The usual plan is to describe various practices and beliefs, which are subsequently discussed

in relation to their probable effects on adult personality. The author shows, for instance, in a striking way how ill-adapted both methods and content of school teaching are to the needs of the Jamaican peasant.

Another section deals with problems of adjustment. Attitudes towards sex and marriage are examined, as well as the frustrations arising from the combined barriers of class and colour. There is a further account of religion, folklore and politics, which Dr Kerr treats as 'projective systems'.

Apart from the data obtained by observation and through informants, the Rorschach and the Loewenfeld Mosaic test were also used. On the basis of these it is claimed that a change in predominant personality type occurs with age. Children 'gradually become more introversive' and their creativeness is stifled by about the age of twelve.

The general conclusion is that 'most of the Jamaican psychological and sociological problems arise out of two culture patterns (Jamaican-African versus Anglo-American) with ideologies which conflict in certain important aspects, leaving the individual bewildered and insecure'. The result is described as a non-integrated basic personality.

This broad thesis is forcefully and, to the present reviewer (who has no first-hand knowledge of Jamaica), convincingly argued. An easy style and frequent quotations from informants make the book enjoyable to read. Dr Kerr must also be congratulated on including a series of delightful lino-cuts in addition to the conventional photographic illustrations.

As a piece of work breaking new ground in social psychology the book has to be judged on more severe standards, however. At first blush one is glad to find apparently all the things psychologists want to know about a different culture, from birth-customs and child-rearing methods onwards. Then one discovers that what is presented are largely generalized pictures of people's beliefs, attitudes and behaviour, with few indications of frequency or extent of deviations from the norm. This is then interpreted in a somewhat speculative manner at a high level of abstraction. An example will make this clearer: we are told that one of the keys to defective personality formation is the fact that households are often without a male head, together with the existence of a cultural ideal of the dominant Victorian father. Consequently 'the child has to introject an idea of the father which is not in accordance with reality. We know that children often in any case introject a phantasy figure built up out of the real figure. But if the real figure is also based on phantasy the position must be extremely difficult. Perhaps for this reason the child does not introject the mother figure as an internal censor as one would expect him to do.'

The question immediately arises in how many households, in what social groups, and for how long, the father is absent. None of this information is provided. There is a lack of even the most elementary demographic data, e.g. the population of the villages where the fieldwork took place. In fairness to Dr Kerr it should be said that she must have had the information at her disposal, as the team of which she was a member attempted a 100% sampling of two of the villages. It is unfortunate that this part of the survey is to be written up separately by somebody else. As it stands the reader is asked to take too much on trust. Beliefs, attitudes and behaviour are incomplete when divorced from their precise socio-economic substratum.

Another point of interest concerns the use of such techniques as the Rorschach, scored according to Anglo-American norms. No reference is made to the fact that such a procedure is at least controversial, especially as the scoring does not seem to have been 'blind'.

It is of course recognized that fieldwork in social psychology presents formidable difficulties. The above criticisms are not intended to belittle the author's achievement, but rather to draw attention to some fundamental methodological problems. Dr Kerr is a sensitive as well as sympathetic observer, and psychologists interested in the study of other cultures should not miss this book. G.J.

L'Année Psychologique. Edited by Henri Piéron. Paris: Presses Universitaires de France. 1951. Pp. vii+592. 2000 francs.

L'Année Psychologique contains a great deal of material of widely different kinds from original articles through reviews and summaries to the proceedings of La Société Française de Psychologie.

Only the original articles will be considered here. One cannot profitably review a series of other reviews. There are eight such articles. The first of them, by Galifret and Piéron, is a traditional reaction time investigation of the speed of response to different signs and intensities of light stimuli round about the threshold value. The curves obtained for mean reaction times at different values present some interesting theoretical problems in the region between psychology and physiology.

The next two articles, one by Stoddard on the development and measurement of intelligence and one by Wechsler on the relation between aptitudes and intelligence, state in outline views which have already

been expressed in the American journals. Stoddard stresses the importance of environmental contributions to intelligence, and Wechsler the necessity for taking into account motivational factors as well as cognitive ones in the interpretation of test performance.

Nony, in an article which is itself something of a review, gives a clear and full account of present evidence regarding the relationship between delinquency and low intelligence. New material is not presented.

Belonging to the same group of articles, in that they are psychological rather than on the borders of physiology, are the reported investigations by Oléron on the sorting test performance of deaf mutes. The crucial nature of this kind of investigation of language and thinking is fairly obvious. Oléron employed two small matched groups preferring Kohs' Blocks to Progressive Matrices as his source of intelligence test scores. He found a significant difference in sorting test performance.

Finally, there are three articles relating to electro-physiological investigation. One is on the G.S.R. by Copelman pointing out, quite briefly, that the G.S.R. can be used in a medical setting for the diagnosis of endocrine dysfunction. The other two, by Gastaut and Povov respectively, present interesting material on the E.E.G. Gastaut is mainly concerned with the evoked potential and Povov with changes brought about by conditioning. In both cases one is impressed at once by the extent of the investigations and by the appalling difficulty of presenting E.E.G. readings in more than a purely descriptive fashion. Dawson's attempt at composite portraiture, reproduced by Gastaut on p. 72, is not particularly successful.

All in all, *L'Année Psychologique* maintains its theoretical-experimental orientation and the high quality of the articles it prints, but one is tempted to complain that rather too much space has been devoted to the marginal regions of the field. JAMES DREVER

Les problèmes fondamentaux de la Psychophysique dans la Science actuelle. By H. PIÉRON. Paris: Hermann. 1951. Pp. 63. (No price given.)

Depressing memories of what psychophysics meant to them as undergraduates, and some natural impatience with discussions about the measurement of sensation, are apt to conceal from many people the notable advances made in this field of recent years. A very considerable body of data has been accumulated which stands in need of fresh attempts at interpretation. Prof. Piéron has done an extremely useful service by publishing the lectures he gave in London in May 1950, in which he collected together in a succinct form a mass of empirical material scattered in widely different places. Thus brought together it cannot but suggest problems of great theoretical interest, and questions of practical import. In so condensed an account much in the way of either methodology or interpretation could not be expected, but this book emphasizes the need that exists for an up-to-date manual of psychophysical problems, methods, and results.

Méthodologie Psychotechnique. By HENRI PIÉRON, PIERRE PICHOT, J.-M. FAVERGE and JEAN STOETZEL. Paris: Presses Universitaires de France. 1952. Pp. viii + 124 to 339. 1000 francs.

'Psychotechnique' is a word with no precise English translation, and on the Continent it has different meanings. For some, it is simply the use of psychological tests in procedures for vocational guidance and vocational selection. For others, it signifies much of the ground covered by the industrial psychologist in this country, while yet a third meaning is wide enough to cover the practice of psychology as opposed to the teaching of the subject. The definition which Prof. Piéron gives in his 'Vocabulaire de la Psychologie' published in 1951 is as follows: 'Discipline qui régit l'application aux problèmes humains des données de la psychophysiologie et de la psychologie expérimentale par l'emploi d'un ensemble de méthodes rigoureusement scientifiques, et principalement des méthodes psychométriques.' One suspects that in the scientific applications of psychology Piéron sees the final refutation of those who in his earlier years maintained the view that psychology was a branch of philosophy.

Méthodologie Psychotechnique is the second part of a work of which seven parts are projected under the general title of *Traité de Psychologie Appliquée*. This volume's four sections are on Experimental Methods and Psychometrics, by Henri Piéron; Methods for the Study of Personality, by P. Pichot; The Use of Statistical Methods, by J.-M. Faverge; and The Study of Opinions and Attitudes by J. Stoetzel. Each section is a short review of its subject, supplemented, except in the case of the section on statistics, by lengthy bibliography. Thus Piéron in 108 pages discusses briefly basic principles of tests and their construction, and then describes, with examples, a large number of types of test categorized by the

aptitudes they are designed to assess. Pichot, Faverge and Stoetzel divide the remaining 100 pages between them.

The book is no doubt designed for students at a fairly early stage of their work. They are given a rapid glance at each subject, and the way to more serious study is pointed via the bibliography. It would have been advisable, perhaps, to have given the student a clear guide as to essential further reading. As it is, he may well become lost in trying to decide for himself which of the very many titles he should tackle.

C. B. FRISBY

Industrial Psychology. (Second edition.) By Joseph Tiffin. London: George Allen and Unwin. 1951. Pp. xxi+553. 35*s*.

This is the issue by a British publisher of the second edition of the book which first appeared in the United States in 1942. The second edition contains a good deal of new material, not only in the form of two new chapters, but throughout the rest of the book. It is difficult to decide for whom the book was written—probably primarily for personnel men in American industry, and secondly for students of courses in industrial psychology. The style is certainly not that of the normal text-book, and the subject matter is rather more concerned with the results obtained from the use of the techniques of the industrial psychologist than with describing the methods in detail, while there is very little on the theoretical principles which underlie the methods. Industrial psychology is described as if it were a branch of 'scientific management', and one cannot avoid the suspicion that one of the author's aims was to 'sell' industrial psychology to the business man.

The largest section of the book deals with the use of tests in employee selection; the dangers of face validity and the importance of validating tests against performance on the job are repeatedly emphasized. The two principal methods of validation, i.e. on existing employees whose performance is known and on novices whose subsequent performance on the job can be compared with that on the tests, are described, but unfortunately in the subsequent references no distinction is made between results achieved by the different methods. The moderate correlations reported for many of the studies quoted are occasionally glossed over with a sentence such as 'However, even with a validity coefficient of 0·26 the test will have real value as a placement device if a sufficiently small selection ratio is utilized'.

In the chapter on Work, Fatigue, and Efficiency, many of the references relate to British work; the author regretfully admits that 'this field, although basically a part of experimental psychology, has been largely developed in the United States by industrial engineering rather than by psychology'. The American examples which he quotes are from the work of engineers such as Gilbreth and Barnes, and he seems to accept the efficiency engineer's doctrine of 'the one best way'.

Despite the differences in outlook and emphasis between industrial psychologists in the U.S.A. and in Great Britain, those in this country will find the book a useful mine from which can be extracted illustrations and references to case studies in the whole field.

C. B. FRISBY

New Means of Studying Color Blindness and Normal Foveal Color Vision. By Gordon L. Walls and Ravenna W. Mathews. University of California Publications in Psychology, Vol. 7, No .1, pp. 1–172, 3 figures in text. University of California Press. 1952. Price $2.50.

This is a report of a study made upon 64 colour normal subjects, 19 protanopic and 16 deuteranopic dichromats, 19 protanomalous and 32 deuteranomalous subjects, 2 monochromats and a small number of relatives of defectives and others. Four tests were used: a filter anomaloscope, a 3-light test for the brightness of colours, a test of the position of the neutral point in defectives and the 'Maxwell's Spot' test.

The authors are enthusiastic supporters of a form of trichromatic theory, and they view all the phenomena of colour vision as fitting into this mould. Consequently their monograph abounds in hypotheses about the way in which the trichromatic theory works, and these add greatly to its interest, though doubtless many readjustments will be required later.

The anomaloscope technique as used in this research could be improved, and it might be adapted in such a way as to make the 3-light test for the brightnesses of red, green and blue unnecessary. By their use of the anomaloscope it appears that the authors fail to make clear some distinctions between types of defectives and of normal subjects which it might reveal if used in an improved way. It would be valuable,

however, if carefully conducted researches could be undertaken to compare the performances of representative groups of subjects, normal and abnormal, on different forms of anomaloscope tests and techniques, so that the most reliable and valid forms could be selected empirically, in the ways used by psychologists with intelligence and other tests.

The weight of interest in the research falls upon its original part, namely the use of Maxwell's Spot, observed with the aid of a purple filter (Wratten 2389) and a neutral filter (Wratten 96) in alternation, by a technique described in the monograph (pp. 67–9). The spot seen is called the RDP (receptor-type distribution pattern), and is claimed by the authors to reveal in all probability six types of normal subjects. It is also used to distinguish the 'collapse' type of defect of the deuteranope and deuteranomalous from the 'loss' type of defect of the protanope and protanomalous subjects, in accordance with the authors' theory of defects. The subject's observation of the RDP, like the observation of after-sensations, is not open to confirmation by the experimenter, and its use as a critical test has limitations and risks owing to its subjectivity. The possibility of other types of defects of colour vision are also considered, with some examples.

The inheritance of colour vision defects is dealt with in an interesting way, and several pedigrees are discussed, including one in which two sisters who are claimed to be identical twins have different colour vision, and two pedigrees for total colour blindness. The two-locus theory is upheld, and the colour vision of hemizygotes (males) carrying two defective genes, one on each locus, is discussed with an illustrative pedigree.

This is a monograph which will excite the interest of all workers on individual differences in colour vision, and should lead to valuable developments. It contains a summary in sixty-one numbered paragraphs and a good bibliography, but unfortunately there is no index. R. W. PICKFORD

The Psychology of Human Learning. By J. A. McGeoch. Second edition, revised by A. L. Irion. London: Longmans, Green and Co. 1952. Pp. xxii+596. 35*s*.

The first edition of McGeoch's book, published posthumously in 1942, was a comprehensive and impartial summary of the great mass of experimental work on human learning. Dr Irion has now revised it to incorporate the findings of the past decade. The revision has been very thorough and has involved not only the addition of much new material and the elimination of some old, but also a considerable alteration in the organization of the whole work. In fact, while McGeoch's general approach is faithfully preserved, this is to a large extent a new book; and Dr Irion is to be congratulated upon the skill with which he has performed his task, which makes this a worthy successor to the original.

The most notable change lies in the greater emphasis on learning theory. This has been introduced at an earlier stage, the second chapter on learning curves being replaced by some general theoretical considerations, while the chapters on Frequency and Effect have been re-modelled and brought forward. An attempt has also been made to relate human to animal learning with a section on conditioning. All this makes for greater interest and for a more logical treatment, but the second edition like the first is painstaking rather than illuminating on theoretical issues. A more sympathetic attitude to field theories would perhaps have helped here. British readers will also regret the absence of any reference to recent work in this country on skill, a topic which this volume rather neglects in general. But all will welcome Dr Irion's concluding plea for greater standardization of conditions in learning experiments, and all will be grateful to him for prolonging and developing the life of so useful and scholarly a book.

R. B. JOYSON

Abnormal Psychology. By D. B. Klein. New York: Henry Holt and Co.; London: George Allen and Unwin. 1951. Pp. xvii+589. 40*s*. net.

This is a revision of Conklin's *Principles of Abnormal Psychology*. In the preface Dr Klein explains his problems and aims in the revision of Conklin's text, and indicates how he has faced them. He considers that abnormal psychology is a wider field and implies a more psychological and a less psychiatric approach than psychotherapy. A book on abnormal psychology should be suitable, he explains, for the training of those who are to become 'well-rounded psychologists and not merely clinical technicians'.

This book is divided into nineteen chapters. The first chapter deals with the scientific approach to abnormal psychology and the concepts of abnormality which will be used. The next two chapters offer

a psychodynamic approach to abnormal behaviour. Three chapters follow in which the applications of this approach to abnormalities of sensation, perception, memory, speech, volition and thinking are considered. There is a chapter on mental deficiency and allied topics. Two chapters are devoted to dreams and to hypnotism and suggestibility; then there are two chapters on psychoneurotic disorders, two on psychotic disorders of functional origin and two on structural psychotic disorders. One chapter reviews the problems of psychopathology as they come before the clinical expert who is interested in establishing a correct diagnosis. This is followed by a chapter dealing with psycho-analytic approaches, one with 'non-directive' and experimental approaches, and one, the last, with physicochemical and surgical approaches to therapy. There is a good index, and the book is written clearly, with a minimum of unnecessary technical jargon. The descriptions of abnormalities, of standpoints, theories and modes of treatment are adequately arranged and sufficiently full to meet the requirements of students beyond the elementary stage who need a bird's-eye view of abnormal psychology.

The general problem of the eclectic position, which is adopted throughout, is raised by this book. The eclectic, without having any special standpoint of his own, undertakes to choose the best aspects of all relevant standpoints, and to make a unified synthesis out of them. There are occasions, however, when these tasks present difficulties of insight and balance which are so great that the resulting synthesis almost obliterates the essentially illuminating qualities of the points of view combined. Many of the difficulties of the eclectic approach arise from failure of intimate understanding of any standpoint. Dr Klein seems to give his allegiance mainly to 'non-directive' or 'client-centred' therapy as the best eclectic technique. It will not take a very thoughtful reader to wonder whether the eclectic approach may tend towards the omission rather than the inclusion of the best, because by being eclectics we almost become outsiders to all standpoints. Instead of attempting the impossible task of setting up an eclectic 'standpoint' it would be better to take a decisive line of one's own and to allow others to choose freely, and it is possible to be fair to other views than one's own without being an eclectic.

Another general problem raised by the book is that of thinking of psychotherapy, and especially of psycho-analysis, as a learning process, and perhaps this view is after all the author's special standpoint. He recognizes or seems to recognize the difficulty in representing this learning process wholly in terms of the de-conditioning and re-conditioning of reflexes, but an extremely wide and generous use of the concept of learning would be required even to identify learning sufficiently with education to make it applicable to psychotherapy in a general way. Then we should have to seek out what special kind of process is involved in psychotherapy as an aspect of the broadest concept of education. According to Dr Klein it is emotional learning.

Much of psychotherapy, however, depends on the reduction of resistances in order to promote developments and maturations of the patient's native endowment which have never been able to take place; much of it takes the form of enabling the patient to readjust and re-distribute his libido investments; much in helping him to gain techniques of thought and language by which he can interpret his motives and those of other people for himself; most of it consists in releasing for active use in sublimated forms those instinctual urges which have been locked up by neurotic demands to ward off and repress fantasies and impulses felt to be dangerous in infancy. If the concept of education can be applied to these and all the other functions of psychotherapy, that concept cannot be represented satisfactorily as learning, unless we run the risk of misleading the reader. The psychotherapist's function is much more like that of the surgeon who makes certain adjustments which enable the patient's own powers of recovery to come into action, than it is like that of the remedial teacher who employs corrective training techniques, even though they depend to some extent on the patient's understanding.

However, this is no place to discuss controversial issues of general interest to psychologists, and Dr Klein's revision of Conklin's book will find a warm place in the hearts of all students of abnormal psychology despite the criticisms which have been made in this review. R. W. PICKFORD

Personality and the Frontal Lobes. By ASENATH PETRIE. London: Routledge and Kegan Paul. 1952. Pp. 188. 21*s*. net.

This book sets out the results of 5 years' research into the effects of both the Standard posterior and the anterior Rostral leucotomy operations. Patients were given a series of objective psychological tests before the operation and at intervals of 3 and 9 months afterwards.

Those who have had experience with research into the psychological effects of physical treatment of mental disorder, are familiar with two major difficulties. The first is concerned with obtaining adequate

control groups. This is usually impossible because if the control group were really comparable to the treated group, they could not, of course, remain untreated. The second problem is concerned with the fact that since the operation is done to improve the patient's mental condition, changes in test score after leucotomy are due, not only to the direct effects of the lesion, but also to the general clinical improvement of the patient. Mrs Petrie has therefore not attempted to test a control group, but has used the results of the standard operation as a base-line against which to evaluate the results from the other types of leucotomy. In order to emphasize the effects directly attributable to the surgical lesion, she has restricted her choice of patient to severe neurotics, all of whom had well-preserved personalities prior to the operation.

Mrs Petrie adopts the dimensions of personality already outlined by Eysenck, namely 'neuroticism' and 'introversion-extraversion', and she has used tests known to be associated with these two dimensions. She begins with a series of hypotheses: that the leucotomy operation would produce a decrease in the degree of neuroticism, a change from introversion to extraversion, a decrease in score on certain types of intelligence test, and that a more anterior operation would produce a similar but less marked pattern of changes. All of these hypotheses were borne out by her results.

These findings are of interest and importance and constitute a valuable contribution to knowledge in this field. They are well set out and the book is easy to read. The general criticisms of the atomistic approach to personality assessment would, of course, apply here, but Mrs Petrie would probably be the first to agree that her results do not tell the whole story.

There is a note in the introduction to the book, to the effect that the statistical analysis was performed by independent computors who were unaware of the relevance of the various scores, and who were therefore not open to suspicion of any *a priori* views. This statement I find completely mystifying, since statistical computation is surely a rigorous mathematical technique which is quite impervious to *a priori* or any other kind of view. The psychologist who *interprets* the results and labels them 'significant' or otherwise, is the person who must meet criticism of this sort, whether he has actually done the statistics or not.

RALPH HETHERINGTON

The Exploration of the Inner World. A Study of Mental Disorder and Religious Experience. By ANTON T. BOISEN. New York: Harper and Bros. 1936, reprinted 1952. Pp. xiv + 322. $4.00.

This is a reprint, with the addition of a brief author's note, of a book published in 1936. At this date the author had been for some years a lecturer at the Chicago Theological Seminary and a chaplain at several mental hospitals. One other most important fact in evaluating this volume is that the author had an acute catatonic episode approximately 16 years before writing it. With his recovery from this, there came to him the core of the thesis of his book.

In the foreword, page ix, the author says: 'It proceeds from the hypothesis that there is an important relationship between acute mental illness of the functional type and those sudden transformations of character so prominent in the history of the Christian Church since the days of Saul of Tarsus. . . . The conclusion follows that certain types of mental disorder and certain types of religious experience are alike attempts at reorganization. The difference lies in the outcome. Where the attempt is successful and some degree of victory is won, it is commonly recognized as religious experience. Where it is unsuccessful or indeterminate, it is commonly spoken of as "insanity".'

The book is divided into three main parts, Part I presents a survey of 173 cases from the dementia praecox project directed by Dr R. G. Hoskins. Here the author relates the reaction patterns of drifting, delusional misrepresentation and panic, to the type of onset, clinical types and amount of religious concern. He then undertakes the always difficult task of psychiatric evaluation of historical characters. The association of the mystical and pathological in the experiences of Saint Paul, George Fox, Bunyan, Swedenborg, Ezekiel and Jeremiah are used to support the argument that religious concern and religious consciousness tend to appear whenever men are facing issues of life and are seeking to become better. Where serious conflict is involved pathological features are also likely to appear.

Part II takes up the problem of Christ as Messiah. Ideas of world catastrophe, cosmic identification, of death and re-birth and mission occur in certain stages of psychoses and in men who are recognized as religious geniuses. They occur in the gospel accounts of Jesus. Disagreeing with Schweitzer, Boisen argues that such ideas do not need to be explained in terms of tradition. They appear regardless of race and time. We might well explain the stream of tradition in terms of the spontaneous resurgence of such ideas. The author pays considerable attention to the topic of ethical values and is critical of its omission from much psychiatric writing.

Part III is devoted to a practical application of one aspect of Boisen's thesis—that the clergy have a clear and important therapeutic role in mental hospitals. Since 1935 he has carried out clinical training for theological students along these lines.

This book is of interest to the clinical psychologist, the psychiatrist and the theologian. Though not of the calibre of James, Starbuck or Leuba, it deserves a place on the shelf beside them. J. C. KENNA

The Psychology of the Occult. By R. H. RAWCLIFFE, with a Foreword by JULIAN HUXLEY. London: Derricke Ridgeway. 1952. Pp. 551. 21*s.* net.

This is a very readable book of which the main interest for psychologists will be the presentation and discussion of a very wide variety of phenomena, ranging from automatic writing to faith healing, from Yoga and other Eastern practices, to experiments on telepathy and extra-sensory perception, from a rationalist point of view.

The writer claims and attempts to show that all the so-called occult phenomena can be explained on normal physiological and psychological principles, and that there is no need therefore to have recourse to paranormal explanations.

With this end in view, he devotes chapters 2 to 6 to an exposition of certain psychological mechanisms and behaviour patterns which he intends to use later to explain various occult phenomena. Many of these patterns of behaviour may seem mysterious to the layman, but are well known to the psychopathologist since they are necessary for the understanding and treatment of the nervous and mentally ill, in whom also they are seen in a more striking form than in the normal individual. This section is long and somewhat cumbersome, partly because it is often too detailed. For example, to realize that hysteria can produce a malfunctioning of any part of the body is important, but to enumerate a long list of such malfunctionings, together with the technical name of each, though interesting, is unnecessary for the purpose of the book and adds nothing to our understanding of its problems. A few examples would have sufficed.

It cannot be denied that this is a book with a mission. We might call it a sort of witch hunt in reverse, in which the hunted are not witches but the believers in witchcraft or rather in magic, under whatever name it masquerades, whether water-divining, spiritualism or paranormal cognition.

The book culminates (chapters 24 to 27) in a discussion of the experiments of Prof. Rhine on extra-sensory perception and of Dr Soal on telepathy. After some criticisms of their work the author shows how the success of those of their experiments which were conducted with adequate scientific safeguards could be explained by sensory cues, given involuntarily by the experimenters and received on the subconscious level by the subjects. This should certainly stimulate any interested readers to go to the original papers and make their own estimates of these conclusions.

Although at times the author is not so rational and scientific as he sets out to be, since he draws definite conclusions where scientifically one could say only that we do not know as yet, and although his thought at times seems rather confused and he has a tendency to shift his ground, as in the chapter on mystical experiences and ecstasy, yet the book is well worth considering seriously, both for its material and for its arguments. A. S. PICKFORD

Paracelsus. Selected writings edited with an introduction by JOLANDE JACOBI, with a foreword by C. G. JUNG. Routledge and Kegan Paul. 1951. Pp. 347. 25*s.*

There is a vast change in thought which now allows the ideas of Paracelsus to be discussed and even accepted. How different from the nineteenth century when such ramblings would have been even ridiculed. No one saw—behind the crude search for gold—the philosopher's stone. Paracelsus's own writings are disjointed and hard to follow, but in this anthology, Miss Jacobi has annotated and chosen with great care. Although she avows in the preface the intention of avoiding specifically medical material she brings out very clearly Paracelsus's own views on doctors of the day—their failings which were many and their virtues which could only be developed by following his methods. 'The physician comes from nature, from nature he is born: only he who receives his experience from nature is a physician, and not he who writes, speaks, and acts with his head and with ratiocinations aimed against nature and her ways. The physician is only the servant of nature, not her master. Therefore it behoves medicine to follow the will of nature' (p. 123). He then proceeds to define somewhat rigorously the qualifications of a good surgeon. These include among them the need for a good moral life. 'He must not be married to a bigot.

He should not be a runaway monk. He should not practise self-abuse.' And perhaps most prejudiced of all 'He must not have a red beard'.

Paracelsus was an extraordinary mixture of the wise and the naïve. He cast the net of learning very wide: 'The physician does not learn everything he must know and master at high colleges alone: from time to time he must consult old women, gypsies, magicians, wayfarers, and all manner of peasant folk and random people, and learn from them: for these have more knowledge about such things than all the high colleges.' It would be fascinating to pursue the analysis of Paracelsus himself from his works, and anyone so inclined will find ample material, grave and gay, in the volume under review. According to Dr Jung, he had a 'luminous inner unity, in whose revealing light his illogical method of exposition, his lack of conceptual clarity, all the seeming incongruities of his psychology and thought appear as tributaries to a broad and powerful stream'.

The quotations are repetitive and dogmatic. Those who are interested in assessing the influence of Paracelsus on the thought of his time will find here much to reward them. There is an excellent glossary of obscure terms.

STELLA CHURCHILL

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